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REVIEWS OF MODERN METEOROLOGY—3

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Dynamics of flow patterns in extra-tropical regions

By E. T. EADY and J. S. SAWYER
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1. Scope

Dynamical meteorology is concerned with the study of the motion of the air, the physical processes (radiation, condensation *etc.*) which are the ultimate cause of this motion, being taken for granted. This study may be pursued in an empirical and descriptive manner and to this extent is included within the scope of synoptic meteorology. Alternatively, or preferably at the same time, it may be pursued theoretically, the aim being to show the causal connection between observed phenomena and to predict new, possibly not yet noticed, relationships. In this article we shall be concerned primarily with the theoretical aspect, advances in synoptic meteorology being described only sufficiently to give a suitable background.

The whole subject of dynamical meteorology as defined above is too vast to be dealt with in one article. We are aware that *scale* plays a very important part in determining the nature of the motion. The motions on a large scale which we infer from synoptic charts are different from the smaller-scale motions inside a cumulus cloud and approximations justifiable in the one case are not so in the other. Because dynamical meteorology is so complicated we are forced to concentrate on one scale of motion at a time. Moreover we may study these motions from two points of view. In the first place we may be interested in the detailed development of one particular situation, as for example the deepening and movement of a depression or the growth and decay of a convection cell. In the second place we may be interested in average or statistical behaviour as, for example, when we study the average effect of disturbances in transferring heat and momentum between latitudes, or when we study the Reynolds stresses associated with turbulent motion. In the present article we shall be concerned only with the first point of view and shall confine our attention to the detailed study of relatively large-scale motions.

Despite this great restriction in scope the subject remains a large one. Neither of the writers feels competent to give a complete résumé of recent work, and the choice of subjects and references to original work is as much determined by their personal interests as by the need for selection. This lack of completeness may not be entirely disadvantageous. Every meteorologist has to be, to some extent, a dynamical meteorologist but not all can afford the time to sift what is new and significant from a spate of papers, filled in some cases with complicated mathematical analysis. More important than the detail is the general trend of recent developments.

The aim of this article is to elucidate some of the basic concepts and lines of thought in modern research. At last, we are beginning to get *all* the data needed for understanding how weather develops. New data have stimulated theoreticians as well as synopticians because in a complicated subject it is a great help to know the kind of behaviour that has to be explained. Moreover the new facts are enabling us to weed out misconceived hypotheses, so that in spite of apparent divergences there is beginning to develop a generally acceptable and relevant body of theory.

2. Recent advances in descriptive aspects of dynamical meteorology

The most notable advances in our knowledge of the movements of the atmosphere have come from the network of radiosonde and radio-wind observations established during and after the Second World War. This network is sufficiently close and extensive to permit the twice-daily construction of isobaric charts for several different levels in the troposphere and lower stratosphere extending over most extra-tropical parts of the northern hemisphere. In some areas more frequent observations are available. These new observations have served to confirm many of the ideas of the structure of depressions established, on the basis of limited numbers of aircraft ascents and ' aerological-day ' observations, by J. Bjerknes, E. Palmén, J. van Mieghem and others. However the new observations have also brought into prominence several aspects of atmospheric flow which were not previously emphasized.

It is found that only part of the thermal contrast between air masses lies at the fronts, the baroclinity, *i.e.* the thermal change in isobaric surfaces, extends into the air masses themselves (Palmén 1948, Palmén and Newton 1948). It is also noted that the direction of the vertical wind shear is usually almost constant through a vertical column. Consequently, when for theoretical purposes it is necessary to adopt a simplified model of the atmosphere, it is possible to consider a baroclinic atmosphere with similar temperature gradients at all levels. This simplification seems to be in direct conflict with the frontal model, but both are useful and the atmosphere is intermediate in structure.

The technique most used in the study of the dynamics of the atmosphere has been that of constant-pressure analysis with particular emphasis on the contour chart of the 500 mb surface. As soon as charts were drawn for a large part of the northern hemisphere several features became apparent (Staff, University of Chicago 1947). It was found that the upper westerly winds were concentrated into a broad sinuous band which extended round the hemisphere in middle latitudes, and was shown on the 500 mb chart by closely packed contours in a wavy band with relatively slack gradients in low latitudes and towards the poles. The troughs in this band were found to be irregularly spaced at intervals usually of from 40 to 100 degrees of longitude (but sometimes more or less) with intervening ridges. These disturbances of the zonal flow have come to be known as *long waves*. It is not possible here to describe or illustrate in detail the behaviour of the long waves, but it is desirable to refer to some of their observed characteristics.

The long waves are slow-moving features of the synoptic chart compared with individual depressions and anticyclones which often appear on the 500 mb charts as relatively small distortions superimposed on the pattern of the long waves. Nevertheless, there is no clear-cut difference of scale – the size of a large depression

r anticyclone in the surface-pressure field is similar to that of one of the shorter long waves' in the 500 mb flow, and the atmosphere appears to be subject to isturbances of any wavelength from a few miles to 3,000 miles or more.

Although more persistent than many individual depressions the long waves arely move regularly or maintain their form over a period of more than 2 or 3 days. hey may increase or decrease in amplitude and new troughs and ridges may be dded to the wave train. Unlike frontal wave depressions which usually go through systematic process of occlusion, the long waves do not systematically increase in mplitude. However, on some occasions their amplitude does become very large nd results in the breaking off from the wave train of anticyclonic eddies to the orth and cyclonic eddies to the south. When a cyclonic eddy is formed to the outh, the main current of westerlies reforms further north, but when an anti-yclonic eddy forms out of a distorted ridge in the flow pattern only a weak branch f the westerlies passes to the south. The formation of such an anticyclonic circu-ation at 500 mb is usually associated with a warm anticyclone on the surface chart; he process is known as *blocking*, because of the interruption of the normal westerly low. Once a *block* of this nature is established it may last for a week or more before he westerlies are resumed (Palmén and Nagler 1949).

Following the suggestion of the Chicago school the mainly westerly current which extends round the hemisphere in middle latitudes has come to be known as he *jet stream*. Rossby has likened it to a meandering river of fast flowing air between elatively stagnant masses to north and south. Cross-sections of the atmosphere Palmén and Newton 1948) drawn north to south through this current have drawn ttention both to the high velocities attained in the upper troposphere and also to he relatively narrow range of height and latitude into which they are concentrated.

Sharp maxima and high velocities are probably not typical of the westerlies n all occasions or around the whole hemisphere. The strongest and most clearly lefined maxima of wind seem to occur in rather straight stretches within the wester-ies. There is some confusion as to whether the name jet stream should be applied o these well defined maxima or to the whole belt of meandering westerlies. The harp wind maxima of the upper troposphere are closely associated with the polar ront. They are situated in the warm air mass. Palmén (1948) has stated that the xis of the jet normally lies above the point where the front reaches the 600-500 mb evel, but there is some evidence that the axis is often above a somewhat higher section f the front. Two explanations of the jet stream have been proposed. Rossby (Staff, University of Chicago 1947) suggested that it could be explained as a result of the nixing by horizontal eddies of the air nearer to the pole under conditions which ended to render uniform the absolute vorticity of the air. Such conditions would esult in a rapid increase of the zonal wind southward culminating in the jet stream outh of which Rossby suggested that mixing transported vorticity from one hemi-phere to the other and resulted in a rapid decrease of wind southward. Rossby's xplanation is directed primarily to explaining the concentrated westerlies as a eature of the general circulation of the atmosphere. On the other hand J. Namias nd P. F. Clapp (1949) were concerned with the local wind maxima when they nterpreted the jet stream as a result of the juxtaposition of airmasses of very different emperatures in a fronto-genetical field. Frontogenesis is an observed feature of the

entrance to most jet streams but the frontogenetic pressure field at low levels may itself be a dynamical consequence of the presence of the jet (Sutcliffe and Forsdyke 1950). Any complete explanation of the jet stream will probably have to treat the high- and low-level wind fields as one three-dimensional dynamical system.

The wealth of available upper-air soundings has also provided the material for the direct study of the fields of divergence and convergence of the horizontal motion and the estimation of the field of large-scale vertical motion. The problem has been approached by different investigators by several methods – the most direct approach, that of evaluating horizontal divergence from wind observations alone has been adopted by H. G. Houghton and J. M. Austin (1946); R. C. Graham (1947) has evaluated vertical velocities thermodynamically, using potential temperature or wet-bulb potential temperature to identify the air parcel; and R. G. Fleagle (1947) has used an essentially similar technique. J. S. Sawyer (1949) has studied the changes of vorticity of a moving air stream (values were approximated on the geostrophic assumption) and evaluated horizontal divergence therefrom. Further insight into atmospheric dynamics has been obtained from comparisons between observed and geostrophic winds (Godson 1950, Bannon 1949).

In all of this work it has been necessary to extract the utmost from the observations. Observational errors and local fluctuations in wind and temperature introduce errors which are comparable with the quantities which are sought. It is therefore highly satisfactory that a consistent picture can be built up from these various modes of attack. Firstly, the average magnitude of the geostrophic departure (root mean square vector-difference) at the 700 mb level seems to be about 7 to 10 kt. (It is probably greater at higher levels). Secondly with regard to vertical movements, the investigators using methods requiring averages over 12 hr and those studying subsidence (S. Petterssen, *et al* 1947) deduce typical values of about 5 cm/sec. On the other hand investigations which involve less smoothing suggest maximum velocities of 10-20 cm/sec in active cyclonic areas and these are confirmed by estimates from rates of rainfall (Bannon 1948). It therefore seems likely that areas of vertical motion exceeding 10 cm/sec exist on a scale of 200-300 mi, much greater than individual convection clouds, but nevertheless affecting rather limited areas on the synoptic scale. The pattern of convergence, divergence and vertical velocity in association with a pressure trough is given by R. G. Fleagle (1947) in cross-sectional form. Ahead of a trough convergence takes place in the lower troposphere and divergence in the upper troposphere and lower stratosphere; in the rear of the trough convergence and divergence are interchanged. Adequate confirmation of the broad features of this picture can be found in other work, but much remains to be done in filling in the details in relation to other more complicated synoptic systems. The material is available for this, but the work involved is lengthy and laborious; moreover it is doubtful if the detail can be improved by present techniques because of the limits imposed by the distance between observations, their errors and by local variations of wind and temperature.

3. Some fundamental problems of atmospheric dynamics

For many years past the equations of motion of a frictionless fluid on the surface of a rotating sphere have formed the starting point for most theoretical studies of

atmospheric motions. There are five fundamental equations which govern the motion, three equations for the components of the motion along the three axes, the equation of continuity, and a thermodynamic relation between pressure and density. Using isobaric co-ordinates these equations can be expressed in the form (see Sutcliffe (1947) for Eqs. (1), (2) & (4))

$$\frac{du}{dt} = -\left(\frac{\partial h}{\partial x}\right)_p + lv \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{dv}{dt} = -\left(\frac{\partial h}{\partial y}\right)_p - lu \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\frac{\partial h}{\partial p} = -\frac{1}{\rho} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\left(\frac{\partial u}{\partial x}\right)_p + \left(\frac{\partial v}{\partial y}\right)_p + \frac{\partial}{\partial p}\left(\frac{dp}{dt}\right) = 0 \quad . \quad . \quad . \quad (4)$$

$$\frac{1}{\rho}\frac{d\rho}{dt} = \frac{1}{p}\left(1 - \frac{R\gamma'}{g}\right)\frac{dp}{dt} \quad . \quad . \quad . \quad . \quad (5)$$

where u, v are components of the velocity along the horizontal axes of x and y. Pressure p is taken as the vertical co-ordinate and the geopotential h is regarded as a function of x, y and p. ρ is the density, g the acceleration due to gravity and γ' the adiabatic lapse rate for dry or saturated air as appropriate. In deriving the equations in the form given above several terms are neglected which can be shown to be insignificant in large-scale atmospheric motions. The motion is nominally referred to rectangular axes with the x, y plane tangential to the earth's surface ; however, the Eqs. (1) to (3) are then only strictly true at the origin. Nevertheless, it is usual to interpret the equations as though referring to a curvilinear set of co-ordinates which conform to the figure of the earth and of which the p-axis is vertical. The Coriolis parameter $l = 2\omega \sin \phi$, then becomes a function of y and this may be important in motion on the scale of the long waves. Further approximations are introduced into Eqs. (1) to (4) when we interpret them with respect to a curved co-ordinate system. McVittie (1949) has shown how the equations of motion can be placed on a fundamentally more sound basis by the strict use of curvilinear co-ordinates. However, there is little reason to believe that the apparent inconsistencies of the rectangular treatment introduce any errors which are large enough to be significant when compared with approximations inevitably introduced in later stages of the work.

More important is the omission from Eqs. (1) to (3) of terms due to friction, because fundamentally the atmospheric motion is the result of a balance between the generation of kinetic energy from potential energy and its dissipation in friction. However, most studies of atmospheric dynamics have been based on the assumption that friction has a relatively small effect on large-scale motions over a period of 24 hr, and this is justified by consideration of the rate of dissipation of kinetic energy.

The fundamental equations have solutions which represent oscillations on several different scales. Thus there are solutions corresponding to sound waves and gravity waves of a few hundred metres length, as well as those representing

oscillations on the synoptic scale. The need to modify the equations to represent only the oscillations on the cyclonic scale has led Charney (1948) to study further the effect of scale on the equations of motion ; *i.e.* the facts that the oscillations which we wish to study have a horizontal extent of 1,000 km or more, a much smaller vertical depth of 10 km and a period of the order of a day. He points out that large-scale atmospheric motion is characterized by approximately geostrophic flow,

$$lu = -\left(\frac{\partial h}{\partial y}\right)_p , \quad lv = \left(\frac{\partial h}{\partial x}\right)_p \text{ approximately} \quad . \quad . \quad (6)$$

Charney proposes to make use of this approximation to simplify Eqs. (1) to (4) and he claims thereby to eliminate the small-scale short-period solutions. However, it is first necessary to modify the equations by expressing them in terms of the vorticity and divergence of the horizontal flow. Thus following Sutcliffe (1947) and omitting some further terms which are believed to be small in large-scale atmospheric motion, we obtain

$$\frac{d}{dt}(\zeta + l) = -(\zeta + l)\,\mathrm{div}_p V \quad . \quad . \quad . \quad . \quad (7)$$

where $\zeta = (\partial v/\partial x)_p - (\partial u/\partial y)_p$, approximately the vertical component of vorticity. Eq. (7) is often referred to as the ' vorticity equation.'

It appears that if we substitute from the geostrophic-wind relation in the expression for the vorticity we obtain a result which is correct to a degree of approximation similar to that of the geostrophic assumption. On the other hand the value of the divergence obtained in this way would always be zero. This is because the divergence is of a smaller order than the vorticity. Nevertheless the divergence is of great importance in the synoptic problem because of its direct connection with the vertical velocity and change of pressure on the moving particle through the equation of continuity (4). Moreover in the vorticity equation, which relates changes in vorticity to the divergence, the vorticity may be approximated by the geostrophic assumption and the equation still gives valid approximations to the divergence. Charney proposes to eliminate the divergence between equations corresponding to (7) and (4) and to substitute for u, v and ζ from the geostrophic condition in the resulting equation. This gives a differential equation involving as variables only the geopotential, h, and the rate of change of pressure on the moving particle, dp/dt. The elimination of dp/dt and the density, ρ, between the resulting equation and Eqs. (4) and (5) is still necessary, but finally a differential equation can be obtained containing only one variable, the geopotential, h. The solution of such a complicated differential equation under appropriate boundary conditions is likely to be difficult, but it does seem that the approach to numerical solution of the equations of motion could follow these lines which recognize the essential characteristics of large-scale atmospheric motion. Up to the present time, however, numerical solution has only been attempted in the essentially simpler case in which the flow is restricted to be horizontal and density changes are ignored (the barotropic model).

The so-called ' tendency equation ' has formed the basis of many discussions of dynamical problems. This equation expresses the fact that the change in pressure at the ground is equal to the change in the weight of air above.

$$\left(\frac{\partial p}{\partial t}\right)_0 = g\int_0^\infty \frac{\partial \rho}{\partial t}dz$$

However, the equation is unsatisfactory for evaluating the important surface-pressure change because it is found that in most synoptic systems the divergence at one level approximately balances the convergence at another (Fleagle 1947). It is indeed a direct consequence of the approximate geostrophic balance and Eq. (7) that surface-pressure changes can only occur with changes of the vorticity of the flow at low levels and these require convergence and divergence much larger than is necessary to produce the change of mass needed to effect the pressure change. Consequently, changes of surface pressure usually occur with compensating convergence and divergence in the column above, and the change of mass in the column is due to a rather small residual divergence.

Attempts made to use the tendency equation as a basis for forecasting are thus unlikely to prove successful. Priestley (1947) also finds this conflict between opposing factors when he attempts to substitute the divergence at several levels into the tendency equation. His divergence has been obtained from the gradient-wind condition. The gradient-wind condition and the tendency equation also form the basis of the largely descriptive account of the dynamics of cyclonic systems given by Bjerknes and Holmboe (1943).

4. Long waves : barotropic theory

Meteorologists have for long been aware of the close relationship between dynamical meteorology and classical hydrodynamics, yet hitherto there have not been very many successful attempts to apply hydrodynamic methods and principles to dynamical meteorology. One of the difficulties has been that the interesting features often depend on the fact that the density is not uniform and then much of classical hydrodynamical theory is inapplicable. Another difficulty is that the motion is three-dimensional and very complicated. Yet it might be that *some* features of atmospheric motion could be studied in an approximate manner by ignoring the actual baroclinic nature of the atmosphere and also the vertical motion.

It was by making sweeping approximations of this sort that Rossby was led to his theory of long waves. The approximations were made tentatively and their justification is that there is in fact *some* resemblance between the theory and observed behaviour. The aim is not a completely satisfactory theory but rather to find a way of making a start towards a realistic theory. With this ' experimental ' attitude let us replace the actual flow by a flow pattern which is similarly distributed in the horizontal but which does not vary with height, *i.e.* by a ' barotropic model.' We ignore the vertical motion so that our model flow is strictly horizontal. Consider the case of a westerly current with a slight sinusoidal oscillation (*i.e.* a uniform current plus a sinusoidal ' small amplitude perturbation '). Now for horizontal barotropic motion it is easily proved from the equations of motion that the vertical component of absolute vorticity ($l + \zeta$) is a conservative quantity, *i.e.* any particle takes its absolute vorticity with it (if we ignore friction). This follows from Eq. (7) by inserting the condition $\mathrm{div}_p V = 0$. Here l, the Coriolis parameter, is the vertical component of vorticity of air at rest on the earth and ζ is the vorticity of the motion relative to the earth. (Vorticity may be thought of as twice the angular rate of

rotation of the air in the immediate neighbourhood of the particle). It follows that as a particle moves towards the pole its relative vorticity must decrease (since l increases) *i.e.* its motion must become less cyclonic or more anticyclonic. Similarly motion towards the equator implies more cyclonic motion. In the case we are considering the vorticity depends only on the curvature of the path (the motion is similar at all latitudes). It is clear then that our assumption of sinusoidal motion is consistent with the vorticity equation, for motion towards the equator leads to a more and more rapid cyclonic turning and eventually the air must begin to move polewards. Later the turning becomes anticyclonic and eventually motion towards the equator again occurs. For one particular wavelength there is also quantitative agreement. If L is the wavelength of the oscillation and U the speed of the westerly current we can satisfy the vorticity equation for a stationary pattern if

$$U = \frac{L^2}{4\pi^2}\frac{dl}{dy} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

If instead of supposing the pattern stationary we allow it to move with speed U_w (*i.e.* the wave velocity) then if U_0 is the mean speed of the westerly current

$$U_0 - U_w = \frac{L^2}{4\pi^2}\frac{dl}{dy} \quad . \quad . \quad . \quad . \quad (9)$$

determines the velocity of a pattern with wavelength L. We note that the waves move more slowly than the mean current.

If we are to apply this barotropic theory to the actual baroclinic atmosphere we must make some hypothesis regarding the appropriate value of U_0 which in practice is a function of height. Since baroclinic long waves extend throughout the troposphere and even some distance into the stratosphere it seems reasonable to interpret U_0 as the *average* zonal flow in this layer, which in practice corresponds roughly to the actual current at about 500 mb. If we observe the motion of apparently well-marked waves at this level we get the impression that they do in fact move with a speed less than that of the mean flow. A more precise, quantitative check is difficult for several reasons. In the first place waves are easily observed only if they have not too small an amplitude so that a theory of small perturbations cannot be quite accurate; also, a practical matter is the fact that simple quasi-sinusoidal waves moving without development are not too common. Moreover troughs sufficiently long (in the N. to S. direction) to justify the application of a formula for infinitely long ones are not observed. In principle the formula can be modified, and troughs which vary in amplitude in the N. to S. direction or which are inclined to the meridian have an effective wavelength different from (normally shorter than) infinite troughs with the same E. to W. wavelength. However in practice this effective wavelength is difficult to estimate. Apart from this the wavelength is usually a function of latitude and it is not obvious whether we should expect the formula to apply at all latitudes or only to mean conditions (averaged over latitude, perhaps weighted in proportion to the amplitude of the wave). There is also great difficulty in estimating U_0. Not only is this sensitive to the exact height which we suppose to be appropriate but this also is a function of latitude and the answer depends greatly on the width of the zone over which we take the mean. It is not surprising that attempts to check Rossby's formula have not led to universal agreement.

It is quite certain that we cannot explain *all* the features of atmospheric motion in terms of a barotropic model. (For example a barotropic atmosphere could not generate kinetic energy and would eventually, owing to the action of friction, come to rest everywhere. Moreover we know that absolute vorticity in the 500 mb surface is not accurately, sometimes not even approximately, conserved). Nevertheless the qualitative success in regard to the behaviour of long waves has encouraged further investigation of the properties of the barotropic model.

One application of barotropic theory is to the waves associated with geographical features such as mountain ranges and plateaux. The effect of an obstruction in an air stream is complex but it is easily shown that an important feature of the resulting flow pattern consists of *stationary* waves. These waves could exist (*i.e.* they satisfy the equations of motion and boundary conditions) if the obstruction were *not* present. Precisely for this reason when the obstacle *is* present these ' components ' must be enormously amplified (as compared with other components) if the boundary conditions are still to be satisfied. In general an obstacle will set off in this way all the possible stationary waves of whatever kind. For example a mountain range will set off ordinary gravitational waves (*lee waves*) with a wavelength of the order of 10 mi. (We shall refer to this problem later). It is evident that the barotropic model is inappropriate for *this* kind of investigation but on the other hand it is evident from Eq. (2) that a different kind of wave (constant-vorticity wave) would also be stationary if the wavelength were suitably adjusted, *i.e.* if Eq. (8) applied with $U = U_0$ the mean flow speed. As a corollary we should expect to see evidence on the upper-air climatic charts that the principal mountain ranges obstructing the flow of the westerlies (the Rocky mountains with parallel ranges and plateaux and the mountain masses and plateaux of E. Asia) are associated with ' long lee waves.' To what extent the observed patterns are to be explained from this point of view is not completely clear, quite apart from the hypothesis inherent in the use of a barotropic model.

The barotropic theory of both travelling and stationary waves may be extended by including another dimension, *i.e.* by abandoning the assumption that the flow pattern is the same at all latitudes. We are then led to more complicated mathematical problems but the solutions are essentially of the same kind as those discussed above.

Another idea which stems from Eq. (9) is that of the *dispersion of wave energy*. If, for simplicity, we retain the assumption that the motion is similar at all latitudes and also that the disturbances may be regarded as small in amplitude, then we may by Fourier analysis split up a complex oscillation into components of various wavelengths. In general there will be components of all wavelengths. Suppose that in the analysis we find a certain *range* of wavelengths dominant. Then our disturbance is to be regarded not as a single wave but as a *wave-packet* (or series of wave-packets). There is an active region (the wave-packet) where the components re-inforce one another and the oscillatory energy is large. Elsewhere the components interfere and the oscillatory energy is small. Now one very interesting feature is the motion of the wave-packet, the region of maximum oscillation, and this in general does not travel at the same speed as any of the component waves. The wave-packet moves with what is called the group velocity U_g and if U_w is the velocity of a component (*i.e.* phase velocity) the group velocity is given by the equation,

$$U_g = U_w - L\frac{dU_w}{dL} \quad . \quad . \quad . \quad . \quad (10)$$

Applying Eq. (2) we obtain :

$$U_g = U_0 + \frac{L^2}{4\pi^2}\frac{dl}{dy} \quad . \quad . \quad . \quad . \quad (11)$$

Thus the velocity of the wave-packet is *greater* than the mean current velocity. Some investigators have inferred that there is evidence of such behaviour in the atmosphere, but as in the case of the individual waves there are difficulties in drawing very definite conclusions. The actual wave-packets are spread over such a wide band of wavelengths that it is difficult both to locate the centre of the packet and to estimate the mean value of L which is the appropriate value to substitute in Eq. (11).

Barotropic theory has been applied in other important investigations. For example Munk has used it to explain many important features of the principal oceanic currents. Of more direct interest to meteorologists is its application to weather forecasting by actual computation, but we shall for the moment postpone discussion of this subject. One further idea may however be mentioned here. We are aware, through the efforts of synopticians, that whatever may be the nature and cause of long waves these do not always maintain their wave character. From time to time the warm tongues and cold troughs become first elongated and later 'cut-off', leading to quasi-independent cold upper cyclonic vortices and warm upper anticyclones. Wave ideas are no longer appropriate and it is interesting to see what barotropic theory would predict about the motion of vortices.

The idea has emerged that cyclonic vortices are subject to an attractive force towards the poles while anticyclones are attracted towards the equator. Without going into details it is easy to see why barotropic constant-vorticity theory could lead to such a result. For a cyclone moving polewards under this law would decrease its *relative* vorticity and therefore its kinetic energy. Since (we are ignoring friction) total energy is conserved there must be a growth in the energy of translation of the vortex, *i.e.* it could be accelerated in the direction of motion. The argument is valid only as long as we can neglect the motion of the air *outside* the vortex, which is not always the case even initially, and is never true indefinitely ; sooner or later the vortex comes to a stop. The protagonists of this view are not always clear on this point. If they had paid more attention to the boundary conditions they would probably have discovered that the final result strongly resembles the wave equation with wave crests and troughs replaced by vortices. There is thus nothing fundamentally new in this idea. Nevertheless it presents barotropic theory in an interesting light. When we observe the growth of a long wave until cutting-off takes place the cyclonic vortices are formed and maintained on the equatorial side while the upper anticyclones appear on the poleward side. This process is exactly opposite to what has been described above. It follows that the barotropic forces on vortices cannot be the only significant ones since in this case they are evidently overpowered. Of course this kind of result should not have been unexpected. It is important to bear it in mind however because it is a reminder to over-enthusiasts of the barotropic model that we cannot hope to explain *all* the principal features of large-scale motion by such means. Rossby has attempted to get over difficulties such as those indicated by postulating a *cold* anticyclone originating in high latitudes. The argument is

eakened because he attempts to take into account changes in potential energy – n other words he effectively abandons the barotropic model. Logically the next tep is to take full account of the baroclinic nature of the motion.

5. Development theories based on the relative divergence

Since the early studies of the free atmosphere by W. H. Dines (1914 and 1925) nd others, it has been recognized that there must be divergence in the upper atmo- phere above a developing depression which (slightly) more than compensates the onvergence at the lower levels. The location of the convergence and divergence re reversed in the anticyclone. Sutcliffe (1939 and 1947) has therefore directed ttention to the *relative divergence*, which he defines as the difference between the ivergence at two levels. It is convenient to consider a simple case of cyclonic evelopment in which divergence takes place in the upper troposphere and con- ergence near the ground. If we then consider two levels, one in the upper tropo- phere and one near the ground, the relative divergence is positive ; in anticyclonic evelopment the relative divergence is negative. It is also easily seen that the relative ivergence is directly related to the large-scale vertical velocity. It is therefore aken as a convenient measure of cyclonic development.

Cyclonic and anticyclonic development defined in this way are not to be re- arded as necessarily leading to the formation of a new pressure centre. Indeed, here are few changes of the surface-pressure field that take place without accompany- ng fields of convergence or divergence (Sutcliffe 1938).

Sutcliffe (1939) first studied the dynamics of relative divergence by considering he difference between the equations of motion at the two levels. His later treatment (Sutcliffe 1947) is based on the vorticity equation and leads to results which can eadily be applied in synoptic practice. Sutcliffe considers the difference between he divergence at two selected pressure levels as given by the vorticity equation in form such as Eq. (7). Like Charney he makes use of the quasi-geostrophic nature of the flow, and assumes that a valid approximation to the divergence is obtained when the geostrophic wind is substituted for the true wind when inserting the vor- icity on the left hand side of (7). Proceeding in this way Sutcliffe obtains the ollowing expression for the relative divergence

$$(\mathrm{div}_p \mathbf{V} - \mathrm{div}_p \mathbf{V}_0) = \frac{1}{l}\left\{ \mathbf{V}_0 . \nabla_p(\zeta_0 + l) - \mathbf{V} . \nabla_p(\zeta + l) \right\} - \frac{1}{l^2}\frac{\partial}{\partial t}\nabla_p{}^2 h' \quad . \qquad (12)$$

where h' is the thickness of the atmospheric layer between the two levels.

Sutcliffe also makes use of the fact that the motion at the two levels is not inde- pendent but, since it is quasi-geostrophic, it is related through the thermal wind equation with the thickness pattern *i.e.* with the field of h'. The changes in h' arise partly from horizontal advection, partly from vertical motion, and partly from non- adiabatic heating and cooling. The importance of these processes for time-intervals not too large is probably in the order indicated. In the simple case in which the changes in thickness are due only to advection, Eq. (12) can be transformed by further use of the geostrophic wind conditions into the simple and convenient form

$$\text{Relative divergence} \equiv \mathrm{div}_p \mathbf{V} - \mathrm{div}_p \mathbf{V}_0 = -\frac{V'}{l}\frac{\partial}{\partial s}(l + 2\zeta_0 + \zeta') \quad . \qquad (13)$$

where V' is the thermal wind, ζ' is its vorticity, and $\partial/\partial s$ denotes differentiation in the direction of the thermal wind.

Leaving aside the relatively small effect due to the Coriolis parameter, $-\frac{V'}{l}\frac{\partial l}{\partial s}$, the expression for the relative divergence separates into two terms which can be interpreted easily in terms of a synoptic chart which carries both contours of the 1,000 mb surface (or surface isobars) and thickness lines for a substantial layer of the troposphere. These terms $-\frac{2V'}{l}\frac{\partial \zeta_0}{\partial s}$ and $-\frac{V'}{l}\frac{\partial \zeta'}{\partial s}$ may be conveniently referred to as the *thermal steering term* and the *thermal development* term respectively.

Consider first the thermal steering term, $-\frac{2V'}{l}\frac{\partial \zeta_0}{\partial s}$, associated with a maximum of cyclonic vorticity, a depression, over which a thermal wind exists. There will be positive relative divergence (from Eq. (13)) and therefore cyclonic development on the side of the depression towards which the thermal wind is directed ; similarly anticyclonic development is expected on the opposite side. It follows that the depression should move in the direction of the thermal wind as is frequently observed. Anticyclones too will tend to be steered in the direction of the thermal wind if as has been assumed the thermal steering term alone is operative.

The remaining term, $-V'\frac{\partial \zeta'}{\partial s}$, the thermal development term, depends only on the configuration of the thickness lines. It makes an important contribution to cyclonic development in certain recognizable situations within the thermal field, notably downstream from the thermal trough and to the left of a difluent region of the thickness pattern.

The general application of these theoretical ideas to the synoptic chart are described by Sutcliffe and Forsdyke (1950). The theoretical ideas suggest convenient rules for the motion and development of individual depressions and anticyclones. A start has been made on the statistical testing of such rules for the motion of anticyclones and the development of secondary depressions on warm fronts and at the point of occlusion (Sawyer 1949b and 1950).

Eq. (13) ignores the effect of dynamical heating and cooling on the thickness pattern. Apart from other approximations the result can therefore only be strictly true if the atmosphere is in neutral static equilibrium. Sumner (1950) has considered the effect of the existence of a stable lapse rate and has shown that it tends to damp down the development that would otherwise be expected. This damping depends on the size of the system ; it is overwhelming for small systems of diameter less than 400 km but is small for large systems of the order of 2,000 km. Consequently, damping by vertical stability may be an essential factor in determining the size of cyclones and anticyclones.

As has been indicated, Eqs. (12) and (13) are valuable in synoptic meteorology and forecasting because they enable the sign of the development and an estimate of its intensity to be made from synoptic charts which can be drawn as routine. The application to forecasting is immediate. Up to the present the possibilities of the numerical application of Eqs. (12) and (13) have not been fully explored, but it is not unreasonable to hope that if numerical values of the relative divergence could

computed on a field basis it would be of great value to the forecaster. Moreover is possible that in combination with a modification of Eq. (7), applicable to a level mid-troposphere, the equivalent barotropic level, Eq. (12) may well form the sis for future work on numerical methods of predicting the surface-pressure field.

Sutcliffe's method is the simplest for examining the properties of atmospheric lotion due to its baroclinic nature. It has the advantage, in common with baropic theory, that it is not necessary to ' linearize ' the equations and we are not stricted to disturbances of small amplitude. However, we always have to pay r simplicity and Sutcliffe's method has the disadvantage that hypotheses are made bout the nature of the motion which cannot be verified any more than can the ypotheses made in applying the barotropic model. This is still the case when the ethod is modified in an attempt to take into account the vertical motion. Sutcliffe's ethod has this in common with Rossby's that both attempt to reproduce three-mensional motion with a two-dimensional model. The vertical structure of the isturbances is not properly taken into account so that, quite apart from other approxiations, it is not surprising that the results are inexact and are approximately true nly in limited sets of circumstances. However useful two-dimensional models nay be it would be an advantage if we could check some of the results against ccurate three-dimensional solutions. This brings us to a consideration of the kind f three-dimensional problem which has as yet been most amenable to solution. The ubject is conveniently approached via the stability problem.

6. The stability of atmospheric motion

The question at issue is whether or not steady baroclinic flow represents a table type of atmospheric motion, *i.e.* whether or not a small disturbance of the low would result in its complete transformation. There is a general method which nay be applied to attempt to solve this type of problem. We assume a small change n the flow pattern (*i.e.* a small perturbation) and on substitution in the equations f motion obtain a set of linear differential equations which determine (with the oundary conditions) how the perturbation will change with time. Usually we annot solve the equations in complete generality but we can study certain particular olutions. Often it is possible to find solutions corresponding to disturbances whose structure does not vary with time. There are normally two possibilities. The disturbance may remain of constant amplitude (though moving), corresponding o *stable wave* solutions. (If frictional terms are included the amplitude decreases lowly with time). Alternatively the solutions may correspond to disturbances which either increase or decrease exponentially with time, corresponding to *unstable waves*. The solutions which decrease are of little interest physically. Those which ncrease will do so until the disturbance is no longer small, and the flow pattern is ransformed. It follows that the original flow pattern is unstable if *any* unstable disturbance can be found ; it is stable only if *all* disturbances are stable. Since the behaviour of a disturbance depends on its structure we ought to examine all possible ypes. Moreover, apart from our interest in the stability problem for its own sake, here is the additional interest that we are led to compute three-dimensional solutions of the equations of motion. Some of these may be typical, in certain circum-

stances, and they provide a useful check on the assumptions made in two-dimensional model theories.

There are two types of disturbance which lead to fairly simple equations. The first type involves circulations in a vertical plane. It may conveniently be studied by the so-called *particle* method.

7. Dynamical instability – particle method

In the full treatment of atmospheric oscillations the variation of pressure must be taken into account and the equation of continuity must be satisfied. However, in the last decade an extensive literature has developed regarding a less realistic type of oscillation in which the pressure field is supposed to remain constant, and in which the motion of one individual particle of the fluid is regarded as independent of all others without consideration of continuity requirements. The results are applicable to oscillations restricted to a vertical plane. The published work on these lines was recently reviewed by one of us (Sawyer 1949a) and references to the original papers will be found there. The criterion for instability of a straight steady current on a rotating earth was first given in full by Solberg (1936) and may be presented in the form

$$\left(\frac{\partial u}{\partial y}\right)_\theta > l \qquad (14)$$

where u is the velocity component along the stream and the y axis is perpendicular to the stream. Thus, for instability the horizontal wind shear across the current when taken along an isentropic surface (θ = constant) must be anticyclonic and exceed the Coriolis parameter l.

If a current is unstable when the pressure is held constant it is likely also to be unstable if this restriction is removed. The criterion (14) is a sufficient condition for instability. It is a necessary condition only if we restrict consideration to instability of this limited type.

If we look for regions of the atmosphere where the condition (14) is satisfied, we find they are very restricted in vertical and horizontal extent. Condition Eq. (14) requires a very strong anticyclonic wind shear, and conclusive evidence for it can only be obtained from a very close network of wind soundings. However, there is evidence that such wind shears are approached on the right hand side of jet streams, and also in frontal regions if the shear is measured along surfaces of constant wet-bulb potential temperature as is appropriate in regions of saturation. The criterion may therefore impose a limit on the shear on the south side of a jet stream, and may also have some importance in the motion in frontal cloud masses, but there is no evidence that it has any direct importance in determining the development of depressions and anticyclones, for which we should expect the criterion to be satisfied over wide areas.

Recently attempts have been made to establish a criterion similar to (14) and applicable to systems of curved flow. However, unless an initial steady flow can be prescribed (and very few simple steady flows are possible) it is difficult to decide what constitutes instability. Recent work by Petterssen (1950) has indicated that the presence of anticyclonic curvature and anticyclonic shear renders a wind

stem more sensitive to disturbances from such causes as pressure changes. Perkins 950) has also given a general treatment of particle motion in an arbitrary pressure eld.

8. Baroclinic disturbances

As we have seen steady baroclinic flow is stable from the point of view of circu- tions in a vertical plane unless the absolute vorticity in the isentropic surfaces negative, so that from *this* point of view and for motion on a large-scale stability the rule rather than the exception. We cannot however infer that the flow is able because we have not yet examined other types of disturbance. There is in ct a second type of disturbance which involves alternating regions of northerly id southerly motion, much more like the initial stages of the large-scale disturbances ten on synoptic charts. These disturbances are usually referred to as *baroclinic aves*. The stability criterion for such disturbances was discovered by Charney 947). He found that the disturbances with a wavelength smaller than a certain itical value can be unstable, but that disturbances with wavelengths greater than ie critical value are stable. On substituting average values for the parameters is criterion indicates stability for disturbances *about* the size of, or larger than, ie average long wave. Judging from subsequent papers Charney appears to have rawn the conclusion that large-scale features are essentially stable phenomena and herefore suitable objects for forecasting by direct computation; we shall refer this question again later. He appears to be less interested in the unstable dis- rbances. Eady (1949) has adopted a different point of view. According to him ie developing unstable waves may represent the initial stages of the mechanism y which solar radiation is transformed (via potential energy) into kinetic energy f the atmosphere. His object therefore has been to analyse their structure and rowth rate for comparison with observation. He found that in most circumstances nstable waves could (and generally would) occur, but that in given circumstances ne particular disturbance should grow faster than any other and dominate the nal pattern. All these unstable disturbances are shorter than the critical wave- ngth found by Charney. The first analysis in fact assumed that the disturbances ere so small that the latitude variation of the Coriolis factor (dl/dy) could be eglected. A second approximation including this factor then showed that the isturbances besides growing should be retarded in the same manner as Rossby's arotropic waves but to a somewhat smaller extent. It is interesting to note that this odification in the wave-retardation factor and also the existence of instability mathematically expressible as an ' imaginary ' wave velocity) both arise from terms nalogous to Sutcliffe's development term. Since Sutcliffe (1951) has subsequently hown that his model implies instability in the circumstances in which Eady's analysis pplies it is possible to use Eady's three-dimensional model as a check in a particular ase on the accuracy with which the two-dimensional models of Sutcliffe and Rossby espectively reflect the actual three-dimensional motion. It appears that while Rossby's model exaggerates the wave retardation (and thus shows quite incorrectly ie growth rate) Sutcliffe's model exaggerates the instability (partly as a result of gnoring vertical motion). Thus both Sutcliffe and Rossby get *one* feature of the motion ery approximately correct. This strongly suggests that a two-dimensional model

combining the features studied by Rossby and Sutcliffe might be of very considerable value, at any rate pending the development of a three-dimensional theory capable of dealing with more general types of motion. One difficulty in three-dimensional theory, even to the extent to which it has as yet been applied, is that it is still necessary to make approximations in order to obtain tractable equations. It is worth noting that Charney and Eady and more recent contributors (Berson (1949), Fjortoft (1950)) make different approximations and approach the problem from rather different angles yet the results are all in broad agreement. This not only increases confidence in the broad correctness of their conclusions but suggests that the essential features are of a sufficiently simple nature to make further generalizations possible.

One may reasonably enquire what practical use may be made of the theoretical results outlined above. So far as Rossby's and Sutcliffe's models are concerned the answer is obvious. Both these models can be and have been applied to actual synoptic problems and, if the results are only of limited value, this is due as much to computational difficulties (or, rather, difficulties in computing the answer fast enough to be of practical forecasting value) and lack of sufficiently accurate or sufficiently detailed data, as to limitations inherent in the crudeness of the model. Before discussing how the computational difficulties may be overcome we may pause to consider the significance of the three-dimensional solutions. From one point of view they represent refinements of the two dimensional solutions and if they could be generalized should give more accurate solutions of similar problems. On the other hand the existence of instability as, apparently, a normal feature of atmospheric motion has led Eady to the conclusion that questions of long-term forecasting can *in principle* only be answered statistically, *i.e.* in terms of the average or most probable behaviour of disturbances. His attempt (Eady 1950) to apply this principle to account for climatic features (such as the general circulation) is outside the scope of this article. Nevertheless it is relevant to point out that, if, in order to solve the equations relating to quasi-stable large-scale motion, we need to include stresses and heat-transfer terms associated with slightly smaller-scale 'turbulent' motions – which cannot be forecast in detail because of the unstable nature of the motion – then we may find it necessary to investigate the laws relating the *transfer properties* of the smaller-scale motion *to the large-scale motion itself*. It is for this reason that Eady emphasizes the importance of investigating the development and transfer properties of disturbances from a theoretical as well as an empirical point of view. It cannot be said that this line of approach has as yet got very far, but neither has the analogous problem in relation to the much smaller-scale turbulence familiar near the earth's surface (though probably existing at all levels) and in aerodynamics. The whole subject of turbulent transfer stands wide open as a challenge to the imagination of the theorist and to the ingenuity of the experimenter and synoptician.

9. Computing the weather forecast

Reverting to more straightforward problems let us consider how a two-dimensional model may be used most effectively. If we abandon the arbitrary simplifications which lead to simple explicit solutions such as Rossby barotropic waves we are faced with the solution of one or more partial differential equations.

we are using a barotropic model we find that the solution of one equation is ficient to determine how the field of motion (or, rather, our model of it) will nge with time. By repeating this integration of the equations of motion at table small intervals of time it is possible to forecast the behaviour of our model, ich we hope will bear some resemblance to the actual behaviour of the atmosphere. t before discussing the details of this process it may be of interest to note some the difficulties involved in computing the weather by *any* method (quite apart m those relating to turbulent transfer, as mentioned above). It will be recalled t just as Babbage invented the automatic digital computing machine long before was possible to construct one, so L. F. Richardson devised a method of computing e weather forecast before the means of carrying out the computations in a reasonable ne existed. The actual construction of an ultra-rapid electronic computer stimu- ed new interest in Richardson's ideas but it has been found desirable to modify s methods. It is possible to write down a system of simultaneous partial differential uations whose solution (assuming that all the necessary data are available) should termine subsequent motion. We cannot hope to solve these equations explicitly t we may hope to solve them by numerical methods (*e.g.* successive approximation, laxation etc.). If we suppose that the terms relating to radiation and surface iction can be estimated, we are faced with a problem which, though complicated, *formally* less formidable than might have been imagined. Nevertheless there e several practical difficulties. In the first place the vertical velocity is not directly easurable nor can it be inferred from the pressure field (only by the *accurate* wind ld) so that initial values must usually be estimated. In the second place the ithmetic involved in solving complicated three-dimensional problems is a formid- le problem even for electronic machines. These difficulties do not rule out the ssibility of computing the weather using quite accurate three-dimensional models t they strongly suggest that we should develop the technique gradually using ite crude models at first to obtain the necessary practical experience. Charney s suggested that we should investigate a hierarchy of models and he has commenced ith what is undoubtedly the simplest, the barotropic model. The goodness or herwise of his solutions is really a matter of secondary importance.

It will suffice to consider only the latest and most ambitious of Charney's ntributions (Charney, *et al.*, 1950) to the computation of the behaviour of the arotropic model. The relevant equation is the vorticity equation (7) which may be -written :

$$-\nabla^2\left(\frac{\partial\psi}{\partial t}\right) = \frac{dl}{dy}\frac{\partial\psi}{\partial x} - \frac{\partial\psi}{\partial y}\frac{\partial}{\partial x}(\nabla^2\psi) + \frac{\partial\psi}{\partial x}\frac{\partial}{\partial y}(\nabla^2\psi)$$

here ψ is the stream function, the velocity components being $-\partial\psi/\partial y$ and $\partial\psi/\partial x$ nd $\nabla^2\psi \equiv \partial^2\psi/\partial x^2 + \partial^2\psi/\partial y^2$, the vorticity. At $t = 0$ the terms on the right- nd side are known so that $\partial\psi/\partial t$ may be determined by solving a Poisson equation ith the appropriate boundary conditions. Strictly speaking the boundary condi- ons can be applied only if we know ψ over the whole of the earth's surface but close enough approximation is possible if we consider only a limited region. oisson's equation is not a difficult one to solve numerically and methods differ nly in the speed with which solutions are obtained in given cases. The method sed by Charney is suitable only for rapid automatic computation using a relatively

fine network of observations. (In this as in most numerical methods the differentials in the formula are replaced by small finite differences). Having solved the equation for $\partial\psi/\partial t$ as a function of x and y we can extrapolate linearly over a small interval of time δt, *i.e.* we replace $\psi_0\,(t=0)$ on the right-hand side by $\psi_0 + \frac{\partial\psi}{\partial t}\,\delta t$. The process of integrating the equation is repeated to give $\partial\psi/\partial t$ at $t = \delta t$, then $\psi\,(t = 2\delta t)$ is computed and so on. In order that errors due to the linear approximation in extrapolating shall not mount up disastrously δt must not be taken too large. (In practice Charney found $\delta t = 3$ hr possible). As is only to be expected, the results of computations of behaviour 24 hr ahead leave much to be desired when compared with the behaviour of the atmosphere. Nevertheless the *feasibility* of computing with a two-dimensional model has been established ; it remains to be seen whether or not more realistic models can be made whose behaviour is sufficiently close to that of the atmosphere to justify computations on a routine basis. In this connection it is of interest to note that the speed of Charney's computations was sufficient barely to keep up with the weather. But it is almost certain that more rapid and efficient methods can be developed.

10. Experiments and dynamical meteorology

Up to now we have confined our attention to theoretical and synoptic work since most recent work (as most of the work in the past) has been along these lines. Although advances in meteorology as a whole have been due as much to experiment as to observation, the kind of dynamical meteorology which deals with motion on a large scale seems an unsuitable field for experiment. We are unable to introduce sufficient new energy into the atmosphere to cause any appreciable modification of the large-scale motion, so that the only possibility for experimental work is the study of the behaviour of models. Unfortunately it is rather difficult to construct anything like a realistic working model of the atmosphere. There is of course some difficulty in scaling down but the real stumbling block is the force of gravity. It is not difficult to reproduce fluid motion on a spherical surface, nor is it difficult to reproduce rotating baroclinic motion, but it is difficult to reproduce the two together, especially with the correct boundary conditions. What we need are body forces of some sort depending on density (not necessarily true gravity) and acting towards a point, the centre of our model sphere. But true gravity acting on our model is so strong that this is practically impossible to realize. If therefore model experiments are to be of value it must be because we realize and allow for the differences between our model and the atmosphere. We may be able to infer *indirectly* some useful facts about baroclinic motion on a sphere *outside* the range in which it is possible to infer facts from synoptic observations. Many meteorologists will be familiar with such experiments as those of Exner in which baroclinic fluid motion was reproduced in a rotating dish by heating at the circumference and cooling with a block of ice at the centre. The result was the genesis of disturbances much like atmospheric cyclones and anticyclones. Experimental work of this sort has tended to be spasmodic, possibly because theory has been insufficiently developed to draw the appropriate conclusions regarding atmospheric motion and to suggest

at experiments should be attempted next. A healthy sign is that the most recent rk, that of Fultz (1949), appears to have been suggested by theoretical work on barotropic model. However naïve or unsatisfactory we may consider the theoreti- background and however inaccurately the model may reproduce atmospheric tion, there is no doubt that the results are interesting and they may be more evant to dynamical meteorology than appears at first sight. Fultz rotated a fluid ween two glass hemispheres (concave upwards) at the same time heating the d from below (*i.e.* at the 'pole'), generating a kind of convective turbulent tion. He observed that an easterly zonal current (*i.e.* one rotating more slowly n the hemispheres) was generated at the top (above about 30°S.) and a westerly hal current at the bottom. It would be rather strange if this reproduction of nospheric zonal motion were purely coincidental, though we cannot make *direct* erences because gravity acts in a different manner in Fultz's model from what it es in the atmosphere. Nevertheless these experimental results may be a pointer regard to the theory of the atmospheric zonal currents since several meteorologists ossby, Starr, Eady *etc.*) have given reasons for abandoning the classical theory of ridional circulations in favour of quasi-horizontal large-scale turbulent transfer angular momentum or vorticity. Their alternative theories would give *similar* ults whether the fluid were heated at the equator (as in the atmosphere) or at the le (as in Fultz's experiments).

11. Topographical effects

In this highly selective account of recent work no mention has been made of rk relating to relatively small-scale phenomena. There is however one small- le feature which may appropriately be mentioned here. This is the modification flow caused by topography. We have already noted the close parallelism between large-scale quasi-horizontal lee waves set off by barriers of continental size and short vertical gravity lee-waves set off by ranges of hills. Lyra (1943) and Queney 48, 1950) have made important contributions to this subject but the most realistic count is perhaps that of Scorer (1949). As has been indicated the lee waves are tionary solutions of the equations of motion which satisfy the boundary conditions the absence of the obstruction and these component disturbances are there atly magnified (theoretically an infinitesimal range of wavelengths is infinitely gnified) when the obstruction exists. The frequency of the wave depends partly the static stability (the vertical gradient of potential temperature) and partly on rate of wind shear. The wave velocity is that of the main current at some height pending on the distribution of wind and static stability. It may be noted that the stence of this phenomenon is not dependent on the rather peculiar wind distri- tions assumed (for mathematical convenience) by Scorer. Lee waves are of nsiderable interest to glider pilots. For dynamical meteorologists they have the ditional interest of being associated with a considerable *form drag* which must be ded to the ordinary skin friction of the earth's surface. It may be noted that *any* d of permanent stationary interference in the flow pattern may be associated with waves. The change of friction at a coast line, for example, is capable of generating m.

12. The future of dynamical meteorology

The present time is a difficult one at which to look forward to the future development of dynamical meteorology because activity on the subject is now much greater than ever before. The most hopeful facts for the future of the subject are that, now for the first time, the dynamical meteorologist has adequate observations which permit him to know the three-dimensional structure of atmospheric systems and which permit him to test his computations. It is a healthy sign that research workers are endeavouring to study and predict the development of actual synoptic situations, albeit by idealising them by restrictive models. This is the work which, if successful, could bring the greatest aid to forecasting, but a very great deal of development and research will be necessary before we can hope to compute tomorrow's weather map. Meanwhile the less ambitious dynamical studies of special models of atmospheric flow will no doubt aid the empirical interpretation and extrapolation of the synoptic chart which is the basis of forecasting – perhaps, more important, they will help to advance meteorology as a science.

Acknowledgment

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Meteorology and Aviation

By SIR ROBERT WATSON-WATT, C.B., F.R.S.

(Presidential Address delivered before Royal Meteorological Society on 16 May 1951)

Let me begin by recalling the story of Airmet. It is a very significant and disturbing story. It well illustrates both faces of the relation between meteorology and aviation. I say story, rather than history, advisedly. For if Airmet has indeed passed into history, if its ' only twenty thousand people – not licence holders of necessity ' (to quote the Assistant Postmaster General) are to be the obscure and unconsidered casualties to an army of, more or less, twelve-and-a-half million more-or-less-listeners, then the relations between meteorology and aviation are in most urgent need of review.

If Airmet were only Airmet one might have hesitated before writing to ' The Times ' about it. When it is discovered to be Seamet and Washmet and Hortimet and Edumet and even Picnimet the hesitation is dissipated. And when it is demonstrated to be Fishmet and Agrimet, and indeed to be more Agrimet than Airmet, the path of duty is clear. Whether ' nation shall speak peace unto nation ' to the exclusion of a valuable aid to the world's primary industry is a national and international problem of real importance – of even greater importance than that of putting ' on the air ' news about the air for those who go up in the air. But is the choice really ' hot news versus cold war ? '

In 1932 the Automobile Association, by its Aviation Division, started radiotelephonic broadcasts of timely meteorological information, primarily for the benefit of owner-pilots. The innovation was thus referred to in the Annual Report for 1931-32, of the Director of the Meteorological Office, presented by the Meteorological Committee to the Air Council :

> Special service for owner-pilots – Whereas a very complete organization for the supply of meteorological information to R.A.F. aerodromes and to civil aviation aerodromes on the continental route, as described above, has been slowly built up, it is found that the needs of owner-pilots are not satisfactorily met, especially when they are about to make long cross-country flights from the numerous aerodromes in different parts of the country. The chief difficulty is that the wireless reports of present weather conditions and the forecasts are issued in the Morse code, and while many private owners have wireless fitted to their aeroplanes, they are unable to receive and interpret Morse messages. To get over this difficulty, the Automobile Association, which has an aviation branch, approached the Air Ministry with a scheme for co-operation to supply information by radio-telephony which could be picked up and utilised by the owner-pilot. As a result of co-operation between the Automobile Association, the General Post Office and the Meteorological Office, a scheme has been worked out, and the first steps to apply it were taken during the year. The Post Office has issued a licence to the wireless station at Heston airport, which has been established by the Automobile Association, permitting them to issue by wireless telephony, such meteorological messages as are provided by the Meteorological Office. The staff in charge of this wireless station intercept certain of the code forecasts issued by the Meteorological Office. These are decoded and from them a message in plain language is built up, giving the information specially required by

the owner-pilot. These messages are then issued by radio-telephony at stated times. Similarly, the meteorological reports which are broadcast hourly from Croydon in code, are intercepted and re-issued in plain language.

These messages issued by Heston can be picked up by aircraft fitted with wireless receiving sets, over a large area round Heston, and in some cases they have been received by means of light portable receivers by aircraft in the air. The wireless station at Heston is the first of a chain of similar stations which it is proposed to erect in different parts of the country, and when the scheme is fully developed, pilots will be able to receive weather reports at any aerodrome in the British Isles and also when in flight in any part of the country.

Three years later the service was taken over by the Meteorological Office, as ıe Annual Report for 1935-36 records :

> METEOROLOGICAL BROADCASTS BY RADIO-TELEPHONY FROM THE AIR MINISTRY – In 1932 the Automobile Association in collaboration with the Meteorological Office commenced a service of weather reports issued by wireless telephony from the aerodrome at Heston for the benefit of civil aviation. An extension of this service to reach the whole country has been under consideration for a number of years. On July 10 1935, the Meteorological Office took over the service from the Automobile Association. From that date the messages have been announced from a special "studio" built in the division and transmitted from the wireless station at Borough Hill, Northamptonshire. The wavelength of the transmission remains the same as at Heston (1,186 m, 253 kc/s) but the power of the transmitter has been considerably increased. The transmissions are heard on a good selective broadcast receiver over most of the British Isles. Reports from certain continental stations were added on September 2 1935, at the request of private pilots who wish to fly across the Channel. On February 15 1936, the whole scheme was reorganized so as to provide full information for both service and civil aviation. Weather reports, weather forecasts, air navigational warnings and weather warnings are now broadcast according to a fixed programme which commences soon after 0700 each morning and continues until 2000 in the winter and 2100 in the summer. The service operates on every day including Saturdays and Sundays. The full programme is printed for convenience of reference as Appendix II to this report.

'his Appendix II was a reproduction of NOTAM No. 14 of the year 1936). No ry strong ground here (or anywhere else) for the Postmaster General's posthumous tation on Airmet, that ' it was not intended for reception by the public generally.'

Interrupted for obvious reasons of security in wartime, it was revived and maintined until 1950. But in March 1950 it was discontinued, on the grounds that no ovision had been made for it in the Copenhagen Plan governing the allocation of ose channels for radio broadcasting, channels that are certainly among the comodities which are in short supply. The Annual Report for 1949-50 records :

> The re-allocation of broadcasting frequencies provided for in the Copenhagen plan was brought into effect on March 15. Owing to the many claims for frequencies within the broadcasting band it was found impossible to make any provision for the Airmet radio-telephony broadcast in this band. The Airmet service therefore temporarily ceased to operate at 1800 GMT on March 14. Evidence of the usefulness of Airmet to aviation, farming and educational interests has continued to accumulate, and it is hoped to restore this service as soon as possible.

Now Airmet was unique among the broadcast weather bulletins of this country, d indeed of the world. The British Broadcasting Corporation devotes four periods, ch of five minutes or less, each weekday to a heroic but certainly misguided attempt

to compress forecasts for the succeeding 12, 24 and 48 hours, for more than two score regions of the United Kingdom and its encircling waters, into a total of just over a tenth of one per cent of its ' Programme Service output.'

Even this modest fraction it treats with a certain measure of stepmotherly aloofness as ' among ' announcements on matters of public importance broadcast at the request of Government Departments. ' Such announcements ' says the Corporation coldly ' include general and specialised weather forecasts . . . . notification of diseases of livestock . . . . ' This makes somewhat obscure the Assistant Postmaster General's statement that the co-operation of the British Broadcasting Corporation was obtained for an alteration of the form of the 5.55 p.m. weather news on the Home service – a co-operation that would doubtless have been accorded if, in fact, it had been necessary. The Corporation's television service presents to viewers once per day a ' present weather ' and a ' forecast weather ' map for the United Kingdom as a whole ; the approximate fifty to a hundred seconds for which each map is visible are generously measured to the naiveté of its content, which in its turn is most modestly measured for meteorological morons.

The only adult weather broadcast by radio-telephony available to the general public was our lamented Airmet, radiated on a wavelength (of 1,224 m) well chosen to give nation-wide coverage by daylight from quite low radiated power. The programme was admirable for aviation, valuable for everyone dependent on the vagaries of our weather. Every hour on the hour it radiated a seven-part programme:

Part	Hour plus min	Contents
1	00	Navigational warnings and unserviceability reports from airfields.
2	10	Plain language statement of the general meteorological situation and the developments expected over the British Isles and adjacent portions of the Continent, followed by weather warnings.
3	20	Description in detail of existing conditions and the changes expected in the immediate future.
4	30	Actual barometer reading (M.S.L.), cloud, visibility and weather at about 40 airfields in the British Isles, and at Paris, Amsterdam and Brussels.
5	40	As Part 3.
6	50	Supplementary actual weather reports, as in part 4.
7	56	Tuning note (1,000 cycles per sec).

The Greenwich Time signal is radiated 00, 15, 30 and 45 mins past each hour.
Upper-air temperature and humidity readings for two stations are announced at 0825 clock time.

It is easy to sum up Airmet as at once too limited and too generous, too closely directed towards aviation needs and too demanding in monopolizing a whole precious radio channel for the whole of the daylight hours. But it is almost equally easy to suggest the quite slight changes that would increase its value to every interested citizen without sacrificing the interests of the aviator. And an open-minded, warm-hearted, un-pedantic, non-highbrow, 'human' review of the content of the other precious channels in daylight hours is far from confirming any suggestion that the

clusive daytime use of one channel for such a service as would be given by a vised 'Airmet' is ill-proportioned to the importance of weather information the conduct of our daily work and play.

By no other means, by no elaborate verbal code giving a specially definite eaning to otherwise indefinite adjectives and adverbs, can the contribution made Airmet to the facilities for appreciating the meteorological situation be achieved. he listener can draw his own adequate, if rudimentary, synoptic chart, and it gives m a firm basis for listening to the duty forecaster at the Central Forecasting Office, unstable, in person. And the sense of intimate, informal personal contact with e mind that had to formulate and assimilate the synoptic survey, and then to base s forecasts on it, is indispensable and irreplaceable. Nothing else can give the er-listener even a fraction of the understanding and informed confidence inspired this person-to-person contact. Nothing comparable could be achieved through e cool clear – because uninformed – detachment of the professional announcer. othing could be in more damning contrast to the infantile 'cat-hat-mat' weather the televised maps, insulting to the intelligences of announcer and viewer alike.

How did we come to lose Airmet – as lost it we have for at least two years, if t in perpetuity? We lost it, of course, because of that national characteristic, of hich we are so inordinately and regrettably proud, of 'just letting things happen.' Ve lost it partly because it was still called Airmet, when it had long ceased to be erely Airmet, though we didn't know by how much it had outgrown its name. Ve lost it because, in circumstances which I propose to recall in a moment, we ntrusted the applications of the science of the air to the Minister for Air – and then iled to notice later that we had no longer a Minister for Air; we had two. We st it because we entrusted to the policeman of the ether, the Postmaster General, nflicting responsibilities. We made him responsible for ensuring that no one eliberately intruded on radio channels allotted to another, we made him also our ternational negotiator for the allocation of the channels which are numerically so adequate to the demand. We endowed him with a kind of abstract proprietorial ide in the content of the programmes which flowed in the allotted channels; but e also endowed him with an economic proprietary interest in some of the channels, hich earned him revenue as a seller of communication facilities. All this we did the almost sacred name of the Postmaster General's Monopoly, now long overdue r close scrutiny and modernization – its centenary is due in 1963. I am bound to nfess that it is not only in relation to meteorology that I am often driven to the feel-g that 'The Monopoly' has become a kind of King Charles's Head at St. Martins Grand – a head that is bloody precisely because it is unbowed!

Am I wrong in seeing the legendary large flat foot of the policeman rather than e traditionally elegant hand of the diplomat or even the hypothetically sagacious ad of the politician in the Assistant Postmaster General's historic declaration of December 1950 about 'a limited requirement in relation to the 12,300,000 oadcast-receiving licence holders in this country . . . . only 20,000 people – not ence holders of necessity – out of a population of, say, 50 million have seen fit sign this petition which, from the point of view of the honourable and gallant ember is all-important'? It would be an excess of politeness to call this a counting heads! But it is certainly no excess of politeness for the Assistant Postmaster

General to suggest that the signatories do not possess receivers for the service they so appreciate, or do not hold licences for the receivers they do possess.

I have much respect for those to whom, with a shade of unintended disrespect due to the differences between the English and American languages, the Assistant Postmaster General refers as radio technicians. He says ' The advice of skilled radio technicians in the Post Office and the British Broadcasting Corporation is that we are in an impossible position.' If only I am allowed to decide the identity of the ' we,' I most cordially agree. But even so it is just plain nonsense to quote a wholly technical opinion as conclusive in a matter which is primarily non-technical. This confusion matches well with another confusion about the meaning of the phrase ' domestic broadcasting ' used by the Assistant Postmaster General. The phrase has no place in the internationally agreed definitions, in which a Broadcasting Service, without other qualifying adjective or adverb, is defined as a ' radio-communication service of transmissions to be received directly by the general public.' A footnote to this only definition of any broadcasting service says ' This service may include transmissions of sounds or transmissions by television, facsimile or other means.' We are thus absolved, at the international official level at least, from discussing the potentially awkward question whether broadcasts, transmitted from the United Kingdom, but explicitly designed to be received outside the United Kingdom, by non-United-Kingdom-nationals, are a part of ' domestic ' broadcasting.

I will not further dissect here the confusing, unhelpful, and in several ways misleading and erroneous statement by the Assistant Postmaster General. Let me merely sum up by saying that we do not submit to Hobson's choice ! And we have I think a right to be told, in detail, why the frequency allocation for Airmet, which appeared in the Berne List up to 1947, which was allowed as a regional assignment in Europe by the very important International Telecommunication Union Conference at Atlantic City in 1947 (subject only to its having been agreed by the Administration concerned) was not defended with vigour at the Copenhagen Conference of 1948 – if indeed it was attacked or even mentioned there. Am I wrong in thinking that the United Kingdom Administration did not in fact submit a claim at Copenhagen for Airmet ? Did our two Ministers for Air let it fall from between them en route to Copenhagen ?

Have we not also a right and a duty to suggest that Finland may not have said its last word on Airmet ? I believe the matter to be sufficiently important for a small *ad hoc* delegation to visit Finland with a request for reconsideration of shared daylight use of Finland's exclusive allocation of 254 kc/s, presenting a case based on the very special interest and utility of a revised airmet and of the readiness with which non-interference can be assured. The necessary work of drafting a convincing and acceptable proposal does not, in my view, present any great difficulty.

If this solution is not attainable – and we could have Airmet again the day after this particular solution was agreed – then we should commend to the authorities concerned the consideration of Airmet as an outstanding example of the British Way of Life – I do not mean its handling since 1947 ; I mean that a broadcast service which shows such maturity and balance in its cultural, educational and practical aspects is in fact better propaganda for the British Way of Life than some of

ıe material that it might so productively displace from Overseas Service trans-
issions by the British Broadcasting Corporation.

And if this very serious suggestion leaves the Air Ministers in particular un-
loved, I would ask them to look at the details of the work the invaluable work –
one by the Uxbridge Air Traffic Control Centre, and see how much labour could
e saved, and how much better the customers could be served, if the new Airmet
ere the main vehicle of advice to these customers amongst many others.

I have thus dwelt on Airmet because of its intrinsic importance. But it is equally
nportant as a synoptic symbol of the interlocking benefits and dangers which
haracterise the relations between meteorology and aviation. Here was a service
ıat existed because it was demanded by private fliers. It was inaugurated and
perated by the organized private fliers in the Automobile Association. When three
ears' experience had shown its value to fliers it was, very properly, and very effec-
vely made a part of the public weather service. But it took the cessation of the
irmet broadcasts to reveal to meteorologists in general – and I suspect that our
ırprise was shared by our colleagues in the Meteorological Office – that they were
ghly valued by a wide range of user-listeners who were not aviator-users. Their
ntent depended for their value, to aviator-user and non-aviator user alike, on a
owth of meteorological science and its applications which owed much to the
imulus of demands from aviators, but perhaps still more to the supply of informa-
on by aviators who act daily, at substantial risk and sometimes with grievous loss,
explorers of the still ill-charted ocean of the air.

I cannot believe that this is a unique or isolated example of our failure to appre-
ate the community of interest and the clash of interest among actual and potential
sers, often insufficiently informed and inarticulate users, of the public weather
rvice. For that reason I propose to cast a retrospective eye on the circumstances
f attachment of the Meteorological Office to the Air Ministry, so that we may
xamine whether a decision that may have been right or wrong over thirty years ago
alls for review now.

My source book is that invaluable if patchy public memory of the Weather
rvice, the series of Annual Reports from which I have already quoted, the ' Annual
eports of the Meteorological Committee to the Lords Commissioners of His
lajesty's Treasury,' which were followed, from the year ended 31 March 1928
clusive by ' Annual Reports of the Director of the Meteorological Office, presented
y the Meteorological Committee to the Air Council.' Parenthetically I may remark
ıat I have found no internal evidence of the reason for this 1928 change, which I
o not regard as insignificant. I believe the relevant extracts on the wider policy
sue to be so important to that review of our public weather service for which I
n to press, that they should be reproduced in our *Quarterly Journal* if, for example,
ou do me the honour of printing there the text of this Address. The published
art of the story begins with the Annual Report for the year ended 31 March 1919 :

Reconstruction

In reply to a request from the Ministry of Reconstruction a memorandum on the organization of the Meteorological Services was presented on November 20 1918, to the Ministry. It embodied, amongst other things, a representation in favour of the establishment of centres for the collection and distribution of meteorological informa-

tion in Scotland, Ireland and elsewhere, which had previously been the subject of communication to Your Lordships. Memoranda setting out the necessary arrangements of a similar character for the supply of information to airmen had also been contributed to Lord Northcliffe's Committee on Commercial Aviation.

PROPOSED TRANSFER OF THE OFFICE TO THE AIR MINISTRY – In the early part of 1918 a proposal was put forward on the part of the newly-formed Air Ministry that the Office should be transferred to that Ministry on account of the importance of meteorology to aviation and the reciprocal importance of aviation to meteorology. At the request of Your Lordships' representative the Director prepared a memorandum setting out the alternative of proceeding on the same lines as heretofore and adding the interests of Aviation to those of the Navy, the Mercantile Marine, Agriculture and other aspects of general economics of the Empire for which the Office is intended to provide. It was pointed out that by regarding the Meteorological Office as a scientific enterprise drawing its information from wherever it was obtainable, from land, or sea, or air, of our own or other countries, and supplementing it by its own observations only in so far as additional facilities were necessary for the adequate supervision and control of the observations, and for the scientific development of the study of them, its primary obligation was to supply the requirements, as far as possible in the present state of the science, of all the various departments of State and of the general public and to obtain whatever information was necessary for that purpose. There remained a residual obligation, with the assistance of the scientific organizations of the country, to use all available means to obtain a greater scientific insight into the atmospheric processes, in order to utilise to the greatest advantage the information which the several departments found it necessary to obtain for their own immediate purposes. The fulfilment of this residual obligation depended mainly upon the ingenuity with which the organization of the Office could utilise the various opportunities afforded sometimes by one department and sometimes by another. If the Office were attached to one department and there were no definite scientific obligation, residual or other, the ultimate objects of the Office in the improvement of the service on the scientific side might be disregarded.

A conference of the representatives of the various departments was held at the Treasury on 31 May 1918. It was decided that it was desirable that the organization of the Office should continue to be independent of the several departments interested in its work, so that its activity might not be restricted by a controlling influence on one side.

Since the close of the year the question has been raised again by the Air Ministry with a different result.

PROPOSAL FOR TRANSFER TO THE DEPARTMENT OF SCIENTIFIC AND INDUSTRIAL RESEARCH – On 4 December 1918, the Committee were notified that, subject to certain conditions the Committee for Home Affairs of the War Cabinet had approved a resolution that the Meteorological Office be attached to the Government through the Committee of the Privy Council which controls the grant for Scientific and Industrial Research, instead of being in direct connexion with the Treasury.

The Committee made careful inquiries of the Department as to the possible effects of the proposed transfer upon the various activities of the Office and the facilities which it enjoys under Your Lordships for dealing with the many miscellaneous questions which are associated in practice with meteorology. They received reassuring replies, and ultimately they felt themselves justified in informing your Lordships that they saw no ground for raising any objection on principle to the proposed transfer if the continuance of the present direct dependence upon Your Lordships was considered to be irregular on general grounds.

Meanwhile, the Committee have themselves raised by letter to Your Lordships the important question of the co-ordination of the various meteorological services in this country to which reference was made on p. 8 of the 13th report. The necessity for this step was made prominent by the transfer of a considerable part of the Naval

Meteorological Service, with its Director, to form a separate Meteorological Service of the Air Ministry. In the absence of proper co-ordination, overlapping, and to some extent, unnecessary rivalry are likely to occur. The Committee have received from the War Cabinet a report of the Research Committee of the Cabinet, in which, at the instance of Your Lordships, the principles of co-ordination are laid down for the guidance of the various services. As this document becomes the instrument upon which the services depend it will be published in due course.

hapter II is found in the next Annual Report, Year ended 31 March 1920 :

The transfer to the Air Ministry

It was recorded in the fourteenth report of the Committee to the Lords Commissioners of His Majesty's Treasury, p. 18, that on 4 December 1918, the Treasury notified the Committee that, subject to certain conditions, the Committee of the War Cabinet for Home Affairs had approved a resolution that the Meteorological Office should be attached to the Government through the Committee of the Privy Council which controls the grant for Scientific and Industrial Research, instead of being in direct connexion with the Treasury.

The question came before the Committee of the Cabinet again on 8 May 1919, on the report of a sub-committee of the Research Committee of the Cabinet, which at the instance of the Lords Commissioners of H.M. Treasury was appointed to lay down certain principles of co-ordination for the guidance of the Meteorological Services. The conclusions of this report are set out in Appendix I. Upon their consideration the Committee of the Cabinet on Home Affairs arrived at the following decision :

> " That the Meteorological Service be co-ordinated on the lines set out in the recommendations of the sub-committee of the Research Committee (R.C.12 G.T.7173) and be attached to the Air Ministry. All other departments including the Meteorological Office to be adequately represented."

The process of attachment of the Meteorological Office to the Air Ministry in pursuance of the decision has been gradual.

After correspondence with the Treasury a conference was held at the Air Ministry on 9 July 1919, at which the Controller-General of Civil Aviation and other representatives of the Air Ministry were present together with Colonel Lyons and the Director of the Office. It was arranged that a letter should be sent to the Treasury suggesting the terms upon which the Meteorological Service of the Air Ministry should be co-ordinated with the Meteorological Office as from 1 July 1919, and the Director undertook to become informally responsible for the Meteorological Service of the Air Ministry, and divide his time between South Kensington and the Offices of the Air Ministry. Accommodation . . . . upon the representation that it was imperative to have at least a co-ordinated service of forecasting, and that the Headquarters of the Meteorological Service should be in the same building with the departments of Civil Aviation, it was arranged that the Air Ministry should devote 9,000 sq. ft of its accommodation for the Headquarters staff and the Forecast Division . . . .

Reconstitution of the Meteorological Committee

The Meteorological Committee continued to carry on its business as before until the Committee was summoned by the Under-Secretary of State for Air (Major-General Seely) to meet at the Air Ministry on 15 October 1919. The meeting was attended by the Under-Secretary, the Controller-General of Civil Aviation, three other representatives of the Air Ministry, the Director of the Meteorological Office and three other members of the Meteorological Committee, also by the Superintendent of the Meteorological Service of the Admiralty. It was thereupon agreed that the Controller-General of Civil Aviation should become President of the Committee, while the Director of the Office remained Chairman, and that the other members of

the Committee should be a representative of the Admiralty, the Ministry of Agriculture, the Board of Trade, the War Office, two representatives of the Royal Society, and two of the Air Ministry (one Secretarial and one Financial). The Colonial Office was also requested to nominate a member, and subsequently a similar invitation was extended to the Royal Society of Edinburgh, in response to a representation presented by the Scottish Office. It was further agreed that the issue of publications including the Annual Report should be on the authority of the Meteorological Committee acting on behalf of the Air Council.

The Treasury also agreed "that as from the date of the transfer the Meteorological Committee appointed under Treasury minute should be wound up, and a new Meteorological Committee under the Air Ministry constituted, in the manner provided in the report of Mr. H. A. L. Fisher's Committee, which was approved by the Cabinet Committee on Home Affairs."

The text of the report by 'Mr. H. A. L. Fisher's Committee' was as under:

We recommend that:

(a) The provision for the Meteorological Office be so increased as to enable it to perform the functions of a central State Meteorological Service.

(b) The Admiralty, the War Office, and the Air Ministry should be represented on the Board of Management of the Meteorological Office with other State Departments for whose operations meteorological information is required, and with members selected in respect of special knowledge of meteorological science.

(c) The Meteorological Office should be removed from South Kensington to some place within easy reach of the Admiralty, the War Office, and the Air Ministry, and should be in direct communication with the General Post Office and a wireless station.

(d) The collection of current meteorological observations should be a general service. Provision should be made for the collection of supplementary area observations required for local or area forecasts. The Air Ministry should have power to establish meteorological stations required in connection with aerial routes, although not otherwise necessary for the general meteorological service. The observations made at such stations should be reported for record and reference at the central Meteorological Office, as ship observations now are.

(e) The Meteorological Office building should be suitable for the accommodation of a service on a much larger scale than at present, and should include rooms for officers detailed by the several War Departments of State for meteorological duties there.

(f) These officers should have ready access to the library and records available at the Meteorological Office for reference, and should carry on their work in due relation to the general service of collection of information for the Office.

(g) The special arrangements for the Meteorological Service which the central office could render to each Department, War or Civil, should be made between the authorities of the Meteorological Office and those of the Department concerned. Departments other than the Meteorological Office should keep that office informed as to any changes in the nature and extent of their meteorological requirements, and should not themselves make provision for these unless it is found that the needs cannot be supplied by the general service.

(h) Current reports and general forecasts for publication as from the central office should be issued only on the authority of the Director of the Meteorological Office; local forecasts and secondary forecasting stations on the authority of the several officers in charge of stations. Forecasts embodying specific statements as to "flying conditions" should be published only on such authority as the Air Ministry directed. Similarly other statements implying expert advice in specific applications of meteorological information should be subject to the authority of the Departments concerned.

(i) The Central State Service embodying the existing Meteorological Office should be, as that office has hitherto been, the channel for international meteorological arrangements.

The Constitution and functions of the Meteorological Committee were revised om their 1905 state to this new form :

1. The Meteorological Committee shall be constituted as follows :

1 – The Controller-General of Civil Aviation (Chairman).
1 – Director of the Meteorological Office (*ex officio*).
2 – representatives nominated by the Air Ministry (including one for finance).
1 – the Hydrographer of the Navy representing the Admiralty.
1 – representative nominated by the War Office.
1 – representative nominated by the Ministry of Agriculture and Fisheries.
1 – representative nominated by the Board of Trade.
1 – representative nominated by the Colonial Office.
2 – representatives nominated by the Royal Society.
1 – representative nominated by the Royal Society of Edinburgh.
(A Secretary shall be appointed by the Chairman).

2. The Director of the Meteorological Office shall be appointed by the Air Council.

3. The Controller-General of Civil Aviation shall act as Chairman of the Committee, and shall be responsible as a member of the Air Council for reporting to that Council. In his absence a Vice-Chairman, elected for the ensuing year by the Committee and selected from among the representatives of the Royal Society and the Royal Society of Edinburgh at the last ordinary meeting in each year, shall take the chair.

4. The members of the Committee shall be appointed by the Air Council, and, subject to the discretion of the authorities by which they are respectively nominated, will hold office for a period not exceeding five years ; but will be eligible for re-appointment.

5. The members of the Committee will not receive remuneration for their services, but travelling and subsistence expenses will be allowed in the case of members not residing in the Metropolis.

6. The Chairman shall call six ordinary meetings each year, and such extra-ordinary meetings as he shall consider necessary. The dates of the ordinary meetings in each year shall be approved at the last ordinary meeting of the preceding year. The notice of any extraordinary meetings together with the Agenda to be transacted at such meeting shall be circulated not less than a week before the date of the meeting.

7. The Committee shall :

(i) Receive the Annual Report prepared by the Director and, if approved, transmit it to the Air Council.

(ii) Consider, and, if approved, transmit to the Air Council the financial programme in connection with the estimates for the coming year, and decide upon any important modifications of the programme which may be found desirable during the course of the year.

(iii) Review the scientific and observational activity of the Office, and consider such proposals as may be submitted to them by the Director for experimental work, the establishment of observatories or stations, and the allocation of the staff to be employed.

(iv) Delegate, by special instructions to be recorded in the minutes, such functions as they may think fit to the Director of the Meteorological Office, or to any sub-committee appointed for a special purpose.

(v) Appoint Assistant Directors and Superintendents, after consideration of the recommendations of the Director.

(vi) Submit annually to the Committee, appointed by the Council of the Royal Society under the Gassiot Trust, proposals respecting the allocation of the income from the Trust for magnetic and meteorological observations or other physical investigations in accordance with the Trust.

5. The Director of the Meteorological Office will act generally as adviser to the Committee on all meteorological and geophysical subjects, and shall be responsible for :–

(i) The administration of the Office and the execution of the duties delegated to him by the Committee.
(ii) The appointment of all members of his staff, other than those referred to in 7 (v) above.
(iii) The issue of all meteorological publications authorized by the Committee.
(iv) Bringing before the Meteorological Committee for their consideration, all matters of importance relating to the application, progress and development of the Science of Meteorology in which the Meteorological Service might share.
(v) Referring to the Finance Department of the Air Ministry proposals for expenditure to be incurred by the Office before submission to the Committee.

This same 1919-20 Annual Report contains a useful summary of the problems of co-ordination into a central State Meteorological Service.

The co-ordination of the services

At the beginning of the financial year 1919-20 the Meteorological services included :

A. The Meteorological Office.
B. The Meteorological Service of the Air Ministry.
C. The Meteorological Section, R.E.
and D. The Meteorological Service of the Admiralty.

In considering the co-ordination of the services it may be convenient here to set out clearly to what extent the separate meteorological services overlapped. The Meteorological Office based its forecasts upon reports from about a hundred stations on the European Continent and the Atlantic Islands supplemented to some extent before the war by ships' observations transmitted by wireless. About 30 of the stations were organised and supported by the Office in order to give a representation of the distribution of the meteorological elements over the Eastern Atlantic and the adjacent continent three times a day. Of these 30 stations seven were observatories where besides the necessary daily observations self-recording instruments were maintained and provision made for researches in association with other countries in meteorology, including the study of the upper air, terrestrial magnetism and seismology. The rest were stations at which the normal daily observations alone were made and reported by telegraph. They occupied the observer's time for not more than an hour and a half each day. The work at these stations was therefore entrusted to coast-guards, lighthouse keepers and others whose regular occupation enabled them to undertake an effective lookout on the weather. The cost to the Office of the whole number of these auxiliary stations did not amount to as much as the maintenance of a single station manned by an officer and men on whole-time service. During the war an additional hour of observation in the small hours of the morning was introduced and this added considerably to the stress of the duty, both at the stations where the observations were made and at the Office where they were received and dealt with.

The development of aviation led to the introduction of observations of wind at different levels by the process of observing the motion of small balloons at the seven observatories of the Office and at the stations of the Naval Air Service. The selection of the stations for the service of the airship patrol naturally resulted in the choice of

localities near the salient points of the coasts, but in many cases far out of the way of land traffic. They required a continuous watch so that they overlapped the ordinary reporting stations of the Meteorological Office which were chosen for a similar reason, but had regard only to the primary meteorological elements which could be observed equally effectively at the new stations. Most of those stations have, however, now been disestablished because the localities have no longer to be chosen for coast patrol. The duplication has practically ceased and now the resources of the coast stations of the Office have to be supplemented in order to supply the additional information which the stations of the Air Force were able to supply as part of their ordinary duty.

More serious overlapping arose from the creation of separate Head-Quarters in London, each provided with a forecasting staff to prepare maps from the information collected in London by telegraph, telephone or wireless, and to interpret them for the guidance of the several services. This was the extent of the overlapping. The separate establishments in different parts of the country and in the theatre of war were not open to that objection because the service for aircraft requires a new type of station for the distribution of information. Up to within a year of the war all the information required for reports and forecasts was collected at the Central Office alone. The observers at the stations had merely to report their readings and were not required to draw any inferences from them. The aeronauts who wished to obtain first-hand information as to the prospects of balloon-flights were accustomed to call at the Office in London and discuss the matter with the officials in charge of the maps. A beginning had indeed been made of the new type of distributive station at South Farnborough, Upavon and Kingsnorth, at each of which an expert meteorologist was placed who was supplied with information from the Central Office and, supplementing it by observations in his own neighbourhood, could advise the local commands upon any meteorological questions which arose. It was the extension of this system to the Western Front and elsewhere that led to the organisation of the Naval Meteorological Service for the Navy and Naval Air Service and of the Meteorological Section, R.E. The tentative plans which were found to succeed during the war have now to be developed into an organised system to satisfy the requirements of aircraft during peace, as well as those of shipping, agriculture and the general public, for which the forecast service was originally created ; and a further development has to be noted. With the facilities for communication that were placed at the disposal of the meteorological staff during the war the collection of information at short intervals as, for example, from hour to hour from a restricted locality proved to be of great practical utility, and now that the air passage from London to Paris is a matter of two hours only the transient changes and chances of weather have become practically a dominant feature of interest for aircraft. Consequently, it has become necessary to provide for hourly reports of weather for the successive stages of the great air routes and their vicinity. This leads to the necessity for the more detailed and intensive study of the phenomena of weather and the inclusion of such new elements as the distance of visibility and its variations, the height and extent of clouds and other details, which were not included in the older reports.

The Annual Report for 1920-21 contains the text of a letter from the Chairman the Meteorological Committee to Sir Napier Shaw, including the significant rase ' carried through the amalgamation of the meteorological services and the orporation of the Meteorological Office in the Civil Aviation Department of the Ministry.' It goes on to remark :

As a part of the Air Ministry the conditions are entirely changed. As a consequence of attachment these independent functions no longer continue and the administration of the Meteorological Office is now definitely joined up with the administration of the Air Ministry : the financial control exercised by the Finance Branches of the Air Ministry is extended to include the expenditure, audit and store accounting of

the Meteorological Office; the appointment of new staff and the rates of pay throughout the Office is examined by the Secretariat of the Air Ministry; the communications of the Office by telegraph, telephone and wireless are arranged through the Controller of Communications; and the demands of the Office on the Stationery Office are unified with those of the rest of the Air Ministry. These are only a few examples of the many consequences of attachment. Such changes have involved, on the part of the Meteorological Office, a large sacrifice of independence, and radical changes in procedure.

The Annual Report 1922-23 records:

> On the reorganization of the Department of Civil Aviation, when the post of Controller-General of Civil Aviation was discontinued, the Air Council decided that the Under-Secretary of State for Air should act as Chairman of the Meteorological Committee in place of the Controller-General; while the Director should report to the Secretary of the Air Ministry on questions of administration.

That is all that we know, officially and corporately, about the operation of co-ordination, the attachment to Air Ministry, and the transfer of the Chairmanship of the Meteorological Committee from the Director of the Meteorological Office to the Controller-General of Civil Aviation and from him to the Under-Secretary of State for Air. There are many obscurities, perhaps related to the state of mind of a cabinet minister concerned, who, at an advanced stage in the decisions, was still unaware that the Meteorological Office and the Royal Meteorological Society were separate entities. But one thing that is not obscure is the insistence on the close attachment of the Meteorological Office to the departments of Civil Aviation. What became of this clear and repeated insistence when the Ministry of Civil Aviation was set up does not emerge from the Annual Report of the period, nor from any published papers known to me.

I referred in my address of last year to the recent converse operation of de-coordination involved in the changed relationship between the Office and the Naval Meteorological Branch – 'coordination' gave way to 'liaison.' I do not resume discussion of this change; I remark that it strengthens the case I am here urging for a public review of the place of the State Meteorological Service in the public service and in the service of the public.

There is one other piece of evidence to be examined before we look at the relative distribution of effort in the Meteorological Office between aviation interests, military and civil, on the one hand and other interests such as agriculture, public supply services, surface transport, and so on, on the other. The Annual Report for 1934-35 records:

> Forecast and aviation division
>
> Since the reorganization of the Office after the war there have been separate divisions for forecasts and aviation, each in charge of a Superintendent. Owing to the fact that the Forecast Division used very largely observations made at aviation stations and to the fact that aviation depended so largely on weather forecasts, it was impossible to draw a line between the work to be allotted to the two Divisions, and in the course of the fifteen years there have been several reorganizations and adjustments. As the result of experience, it has been decided that the most harmonious and efficient method of work would be to combine the two Divisions under one head with two senior officers as deputies who would be respectively in charge of the two branches of the work. On October 1 1934, the initial step in the reorganization was made, the

two Divisions were combined and Mr. Corless, the previous Superintendent of the Forecast Division, was placed in charge of the combined Division with Mr. H. W. L. Absalom as his first deputy.

This change was certainly justified on immediate convenience and expediency; s for consideration whether it did not reinforce the tendency to exaggerate the portance attached to the undoubtedly important aviation interests in the total ntèle of the Office, and whether the organisational convenience of the plan was bought at too great a price in unbalance of service to other clients, and in parti- ır by increased difficulties in showing the non-aviation client how much more should be demanding from the Office. This unbalance may well have persisted r the temporary merger was dissolved by the further development of the teorological Office organisation.

I have, by implication at least, admitted to having been surprised by the results he valuable if limited piece of 'Market Research' which was planned when iatories to the Airmet petition were invited to 'declare their interest.' The sur- e must not, however, be interpreted as having gone beyond the limit of the in- igation. It would be surprising if farmers were not more continuously interested hort-term forecasts than are aviators; it was a little surprising that they actually numbered aviators in their support for an ostensibly aeronautical forecast service.

How does it happen that the aviators, having still got less weather service than y need, have got so much more relatively than have the other clients? I have de no effort to discover how much more, but I venture a guess that they take the ater part of the time of three-quarters or more of the total staff of the State teorological Service, plus that of a very substantial majority of the staff of the val Meteorological Branch. They certainly have an overwhelming advantage being, by and large, the only customers who find their meteorological suppliers heir elbows, for the great majority of the distributive centres for the public ther service are located at military or civil aviation centres. The aviators are e congratulated on their persistence and their success in obtaining these facili- ; the non-aviators are to be condemned rather than commiserated with on having n less successful because less demanding.

They would not, in fact, have had even that fractional service which they now by had the aviators not been exacting. The whole of modern synoptic meteorology ld have looked different but for the radiosonde which was made necessary by aviators' demands for upper-air information. The forecast for the farmer to-day ld be far less dependable were it not for the weather ships in the Atlantic de- nded by the aviators. Within my own special and limited spheres of activity demands of the aviator gave meteorology its 'Spherics' service, its radar wind- ing service, and the tools for the intimate study by radar, of the processes of d and precipitation, and perhaps of ice-deposition.

Looked at more generally, our own civilization grew up on the Atlantic sea- d in the fifties of latitude, and took the form it has, largely because it grew in cockpit of meteorology, the age-old battleground between the great polar air s and the tropical and maritime air streams. Our meteorology helped to make what we are, and we spelled out the simpler laws of that meteorology as we hed the battle. Now our aviators have chosen as their preferred field of opera-

tions another region of recurrent discontinuities; they want to fly systematically at heights where, less systematically, the tropopause surges upward and downward to the accompaniment of dramatic phenomena such as jet streams about which they want to know more. And we now know that the more we learn about the meteorological battlefield aloft the more we shall know about the cockpit below.

We shall not have that public weather service which is justified by the potential economic value of timely weather forecasts until there is a weather salesman at a public 'counter' behind an attractive and arresting 'shop front' in as many market places, agricultural and commercial, as there now are airfields. And we shall not have that until a force of well-informed uncommercial travellers in meteorology have 'sold' the economics of weather to the major industries of the country, beginning with agriculture. And we cannot do that with a ten-cent weather service.

Ad interim we might do something towards breaking into this closed ring of the *non possumus* (we must have a more expensive weather service before we can show the citizen how much better off he would be with a still more expensive service) by the introduction of such an automatic telephonic weather service as I have used in the United States (where a third of a million calls a day are dialled to 'Weather' in seven cities) and such as already existed many years ago in continental Europe. It would be far from sufficient to be able to dial WEA; it would be *déjà quelque-chose*, especially while we wait for our restored, rejuvenated and widened Airmet. It is not sufficient, I say, to have an automatic telephone weather service; it is certainly very far from sufficient to have those limited and expensive facilities for telephoning (always subject to the sanction of the 'engaged' signal) to 'the nearest meteorological office' on which the Assistant Postmaster General relies for his conclusion that 'farmers, or even people who want to know about the weather for social purposes,' are not 'suffering any great distress or inconvenience.' If the Office wants a slogan, I offer it 'Meteorology in the service of productivity.'

When I plead for a re-examination of the best mode of attachment, in present circumstances, of the State Meteorological Service to the organs of government, I am mindful alike of a resolution passed by this Society on 18 February 1920 and of an appeal for another look at the matter which was made by C. J. P. Cave in mid 1949. The resolution ran as follows:

> The Royal Meteorological Society observe that in the Air Estimates for 1919-20 published last December there appears a sum of £12,000 as a supplement to the grant-in-aid of the Meteorological Office. It would appear from this that it is intended that the finances of the Meteorological Office shall pass under the control of the Air Ministry.
>
> The Meteorological Office deals with a variety of problems of high scientific and practical importance, some of which have no bearing on the work of the Air Ministry, but are closely connected with the work of other Government Departments. While recognizing to the full the great benefits to the meteorology of the upper air likely to accrue from a close association with the service to which a knowledge of the upper air is so essential, and which possesses such facilities for its investigation, this Society cannot but feel misgiving that there may be a tendency for other branches of meteorology to receive less than their due attention if one Government Department has the sole control of the finances and management. The Society therefore are of opinion that the Meteorological Committee should continue to have full control of the expenditure.

I am mindful, too, of the Editorial in 'Weather' for April 1949 which posed the very pertinent question 'Of the many "interests" which make use of the

ernment meteorological services, which group should be considered first' ? e editorial continued thus :

It was the seamen who first persuaded Treasury to spend money on meteorology, but airmen now have the biggest say in determining the policy of the Meteorological Office, which since 1920 has been a department of the Air Ministry.

The requirements of agriculture were discussed last month at meetings of the Royal Meteorological Society in Edinburgh and London. The claim was made that farmers have more to gain than anyone else from a suitable meteorological service, and that the growing of food is more important than sending an aircraft from A to B. Considering that agriculture is our greatest industry – producing more worth of goods than the coal mines and the iron and steel industry together, and one and a half times as much as the navy and air force spend between them – this claim deserves to be taken seriously. With shortages of labour and the increasing size of his operations, the farmer is having to plan his work more carefully and further ahead than ever before.

The repercussions of an unlucky decision or a missed opportunity persist throughout the whole season.

If it is really true that weather forecasting and research have important parts to play in increasing our crops and reducing their cost of production, then perhaps it would be wise to consider once again the setting up of a state meteorological organization, independent of the armed services and of all sectional interests. It would be tragic if what began as a protection for mariners against the dangers of foul weather should come to be regarded largely as a weapon of war.

And, finally in this group of quotations from our stimulating younger journal, ve's last paragraph :

Some would argue that such vast sums as are now paid for the Meteorological Service would have been impossible had the Meteorological Office not been taken over by the Air Ministry. Those who argue so I would refer to *Genesis*, Chapter 25, verses 29 to 34.

Since I have been diligent in avoiding cross-references, I quote *in extenso* passage here cited :

And Jacob sod pottage : and Esau came from the field, and he was faint :

And Esau said to Jacob, Feed me, I pray thee, with that same red pottage ; for I am faint : therefore was his name called Edom.

And Jacob said, Sell me this day thy birthright.

And Esau said, Behold, I am at the point to die : and what profit shall this birthright do to me ?

And Jacob said, Swear to me this day ; and he sware unto him : and he sold his birthright unto Jacob.

Then Jacob gave Esau bread and pottage of lentiles ; and he did eat and drink, and rose up, and went his way : thus Esau despised his birthright.

It is to be observed that in discussing this subject or group of subjects I have consulted the man who knows most about it, the Director of the Meteorological ice, and I have not consulted the Council of this Society. In both cases I have it desirable to avoid the embarrassment to some of our most important members ch might result from inviting them to comment before the Society as a whole the problem presented to it. But the absence of fully-informed opinion does deter me from making certain specific proposals for action.

I believe it to be a large part of the duty of this Society to encourage recognition he economic importance of timely short-term weather forecasts in industry, and

to do this industry by industry. To that end I propose that the Society should set up Interest Groups, beginning with some of the following, *viz* : (a) Agriculture and Horticulture, (b) Fishing and Little Ships, (c) Merchant Navy, (d) Education, (e) Sports and Entertainments, (f) Road Transport, (g) Engineering, (h) Food, (i) Distribution, (j) Non-scheduled Flying, (k) Domestic and Social.

I know of no other way in which the knowledge of what is needed by a special group of users, and the knowledge of what is likely to be attainable to meet these needs, might be made available to them. I know, too, that the effort will be wasted unless each group finds for itself, from within or without, a well-informed enthusiast to be an effective middleman, stimulating each side to useful exploration and to effective action. Let no one hasten to remind me that many of these ' interests ' are represented at or near the exalted level of the Meteorological Committee itself by distinguished civil servants from user ministries. In my thirty years as a lesser bureaucrat I learnt some distrust of high-level representation, not of its good intentions but of its understanding of the undistinguished mortal and his daily needs. I am all for more zeal than discretion !

And I am all for such low-level informalities as will discourage no honest participant from speaking lest he uncover his ignorance. I have learnt from Operational Research in warfare, and from a scientific advisory practice in peacetime, that what the client asks for is not always that which he most needs ; that what he might have for the asking is often better than what he knows enough to ask for, and that an honest but persistent broker is of the essence of the applications of science to daily life.

Here then are three targets for our efforts to match meteorology to man's needs : (1) a broadcast weather service like Airmet but designed to serve all industry, attainable by prosecuting a case to be formulated by a small expert committee preferably under a Chairman nominated by the Society, (2) a public enquiry into the best attainable organization of the State Meteorological Service in the light of the changes within itself during its thirty-year attachment to an Air Ministry which has also greatly changed and (3) Interest Groups within the Society to help match the activities of the State service to the needs of the citizens, who *are* the State.

I know there are a lot of committees. But what can be achieved without committees of those who know ?

SIR ROBERT WATSON-WATT, C.B., F.R.S.

President of the Royal Meteorological Society, 1949-1951

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An essay on the general circulation of the atmosphere over South-East Asia and the West Pacific

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(Manuscript received 19 May 1950, in revised form 30 April 1951)

Summary

The major airflows between the surface and 10,000 ft are studied for the area between Japan d the equator and longitudes 100°E. to 150°E., and a description is given of upper-level influences ich have not been apparent hitherto since only surface analyses were prepared before World War II. nvergence zones between airflows near the equator take the place of the fronts of high latitudes.

The traditional theory of the intertropical front of summer is inapplicable to the region and a w concept, developed from observations, is described of an impermanent intertropical convergence ne formed by the occurrence of localized clockwise eddies in equatorial regions which bring southern misphere air across the equator and into contact with that to the north. If considered in relation the West Pacific the traditional intertropical front of India is shown to be in reality part of the est Pacific polar front.

1. General

This analysis is based on a study of the daily flow charts for the levels 2,000, 000, and 10,000 ft prepared at the Royal Observatory, Hong Kong, since September 947. Of these the 10,000 ft chart contains by far the largest amount of information, lot-balloon observations being supplemented by weather reports from transport rcraft flying at approximately this height, but by itself, of course, it fails to give complete interpretation of the observed weather sequence and must always be used conjunction with the lower-level charts.

At the present time the only direct approaches to the problem of forecasting the area are through the surface and airflow charts. Radiosonde ascents have been ade in Japan and at Manila and Guam since the war and have recently commenced Hong Kong, but the density is far too sparse to allow the preparation of isobaric- ontour charts or other innovations. In general pilot-balloon information has been nproving steadily since 1947 but even today is sadly insufficient. Thanks are due many transport aircrews for their diligent weather reporting. Their wind bservations have been mainly used in this survey and experience shows a high egree of similarity amongst the separate reports when the density is great, but ccuracy is not sufficiently good to permit a close and detailed study over small reas, nor should single aircraft winds be accepted too readily unless they conform ith the known features of the airflow, or can be checked in some other way. One f the gravest difficulties of the analyst is to decide the value of observations. The nusual or unexpected report, which may be the key to a situation, is treated warily nd is often discarded, the airflow chart being accordingly oversimplified. On the ther hand the treatment of observations with equal respect renders the charts ometimes impossible to draw to any pattern at all, or hopelessly over-complicates he picture. The problem is made no easier by the scarcity of reports from China

* Now at Colonial Insecticide Research Unit, Arusha, Tanganyika.

where a pilot balloon or an aircraft wind can be the signal, unless impulses are strongly checked, for a variety of imaginative streamlines over the whole country. Bearing in mind the vastness of the area and the scarcity of reports it is hopeless to attempt to make the streamlines fit exactly the reported winds. Minor irregularities in airflow must perforce be neglected and streamlines drawn to accord with the general large-scale pattern.

For each of the sections describing the events of a particular season a model chart is given illustrating the main airflows and their convergence zones. These are neither mean nor actual charts ; they exemplify the air flow distributions most commonly occurring and are presented solely to illustrate the main features of the seasonal flows. They are designed for their synoptic application and attempt to represent the background of the seasonal daily synoptic situations in so far as one or two charts can possibly do so. In practice the synoptician should attempt to locate these seasonal flows and their convergence zones and assess their intensities and interaction ; naturally they will not always be found with the strengths and positions depicted on the model charts. However for the majority of days of a season the daily flow charts are very similar to the appropriate model chart. Perhaps the most striking feature of the study was the small change of the major flow patterns during each season, except during the transition period of May.

2. Streamlines and convergence zones

Hong Kong upper-air charts are drawn in accordance with principles of streamline analysis gradually evolved in tropical areas where there are fewer radiosonde ascents than in higher latitudes. Streamlines on the charts presented in this study are drawn in the usual way tangential to the reported winds, but speed is only roughly indicated by spacing, it being rarely possible to do more than this in practice, since the information on which daily charts are based covers a period of 12 hr, almost invariably the daylight hours.

Concentration of attention upon the wind field rather than the temperature field has led to the introduction of the term *convergence zone* to describe the boundary between approaching airflows of different histories. Such features are analogous to the fronts of high latitudes except that since upper-temperature observations are not generally available it cannot be proved in all cases that density discontinuity exists between the two airflows, nor do the normal models of weather at warm and cold fronts apply to convergence zones within the tropics. Thus, until our information is more complete, it is preferred to retain the term *convergence zone*, which describes what is observed, rather than use the word *front* which tends to imply specific characteristics.

Experience shows that the convergence zones between the major airflows of the region, where the convergence is normally throughout a deep layer, coincide with belts of cloud and precipitation, and hence ascent of air may be assumed. A streamline chart showing a moist layer developed at such a convergence zone is given in Berry, Bollay and Beers (1945) where also the modern technique of streamline analysis developed in the Americas is summarized. The weather at an active convergence zone can be very bad. Heavy rain from thick altostratus cloud may

cur, associated sometimes, but not always, with thunderstorm activity. Little known of the structure of these systems. At times aircrews report two parallel es of violent cumulonimbus separated by thick altostratus, occasionally one line cumulonimbus and associated altostratus, at other times there may be no cumuimbus at all. There is however considerable diurnal variation in intensity of th the cloud systems and the rainfall, and there is also much variation of the width the belt of bad weather.

The convergence zones occurring between those major airflows of the region hich have had a long trajectory near the equator, and hence have probably acquired nilar temperature characteristics, tend to be, in their season, near-permanent atures, and have no smooth movement. Forsdyke (1948) has stated that convernce zones often move discontinuously. This is agreed with the proviso that, this area at any rate, major convergence zones do not appear to move and at the me time retain their bad weather characteristics. Normally a convergence zone ay be active for a few days along one particular line but thereafter it will weaken disappear altogether in accordance with temporary changes of the contributory flows. Later, in a new position, though still oriented approximately parallel to previous line, it will re-energize and a fresh period of disturbed weather results. is better therefore, at any locality in the tropics in this area when bad weather ie to such a major convergence zone begins to appear, to forecast a continuance the weather rather than a movement away of the system. Similarly when weather good no sudden change by the arrival of a major convergence zone need be excted. Forecasting on a persistence basis is thus to a large extent justified. It is nphasized that these remarks apply to the convergence zones between those major flows which appear to have acquired similar temperature characteristics by long ajectory over heated regions. They do not apply in the case of the convergence sociated with troughs moving within an air-stream, nor in the case of polar outeaks into the tropics, where density discontinuity may be safely assumed. It is ggested therefore, though admittedly with no strong evidence, that a major nvergence zone will travel in a direction normal to itself only if it has some gree of density discontinuity.

Winter (November to March) – the domination of continental air

(a) 2,000 *ft*

Fig. 1 shows the relation between the two air masses, continental and Pacific aritime. Surface charts are dominated by the great Siberian anticyclone centred ar Lake Baikal. The system is shallow and best imagined as a blanket of exceedgly cold air covering Siberia and a wide extent of the peripheral regions. The nticyclone is subject to considerable variations in intensity, a period of intensifition being associated with a fresh outpouring of a shallow layer of cold air which reams southwards over Manchuria and China, its leading edge being a new W. acific polar front. Over the Pacific south of Japan this cold Siberian air contacts ades flowing under the influence of the sub-tropical Pacific anticyclone, and the ont is accordingly often strong and active. Over and off S. China the Siberian r has a direction from the north-east and meets trades flowing westwards across ndo-China into the Indian Ocean, no occasion having been observed when Siberian

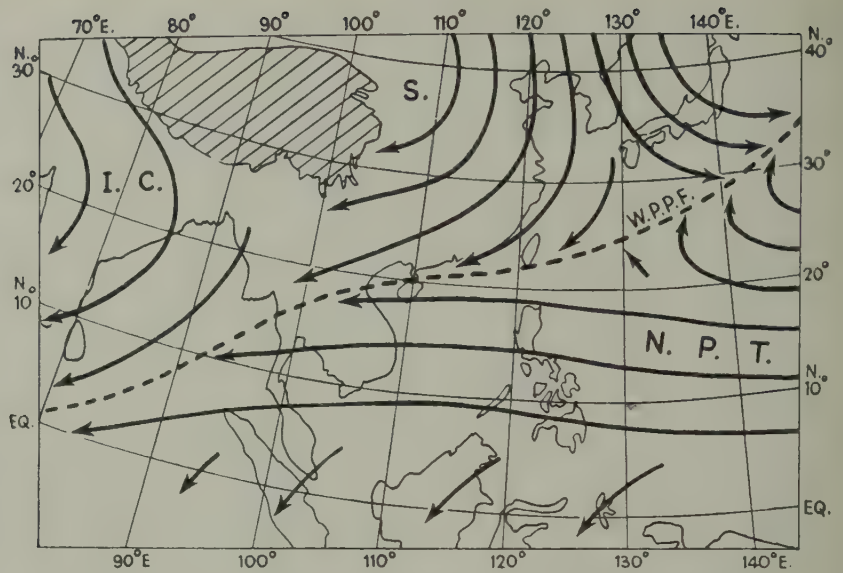


Figure 1. The most common flow pattern at 2,000 ft from Nov. to Mar.

I.C. – Indian continental, N.P.T. – N. Pacific Trades, S. – Siberian, W.P.P.F. – W. Pacific polar front
The Tibetan Plateau above 3,000 m is hachured.

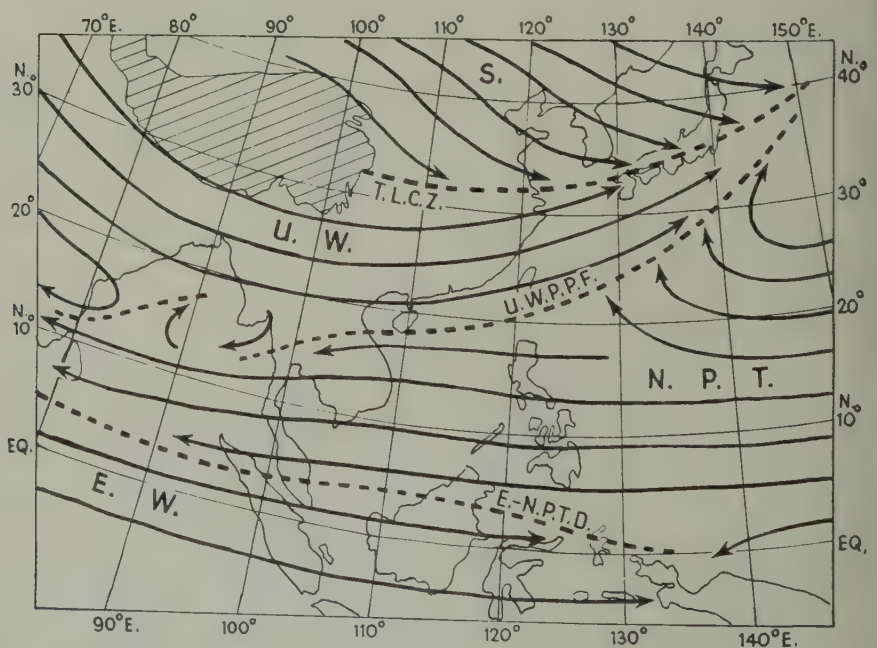


Figure 2. The most common flows at 10,000 ft from Nov. to Mar.

U.W. – Upper westerlies, E.W. – Equatorial westerlies. T.L.C.Z. – Tibetan Lee convergence zone, U.W.P.P.F. – Upper W. Pacific polar front, E. - N.P.T.D. – Equatorial – N. Pacific trades discontinuity.

ir penetrated farther south than southern Indo-China. The front in this section herefore has a different structure and, when the Siberian flow is weak and has traersed a wide extent of the ocean, becomes almost undetectable. Over the Indian Ocean the front is always weak and indistinct, though continuing to be a separation etween trades and continental air, the latter however being not an air mass which as traversed China but the feeble, probably subsided, northerly of India. The ront has no importance over the Indian Ocean.

(b) 10,000 *ft*

There is considerable difference between the flow at this level, shown in Fig. 2, nd that at 2,000 ft. In the north, strong westerly flows predominate instead of he northerlies of the lower level; the Siberian anticyclone does not extend to 0,000 ft. The transition from the one régime to the other is abrupt, especially ver S. China.

Siberian westerlies occur north of the Tibetan plateau but the flow acquires a legree of more northerly component over Manchuria and N. China, probably due o a tendency to spread on reaching the eastern edge of the plateau, and because of he influence of the pressure pattern developed aloft by the near-permanent Aleutian ow, or other common lows, in the Japan area. This airflow shows much less varia- ion in direction and speed than does that at 10,000 ft over Great Britain. Circumstances are different, the Siberian anticyclone is a buffer to depression activity and the upper-level winds are consequently smoother. The Tibetan lee convergence one is formed on the southern boundary of the Siberian westerlies where the low converges over China with upper westerlies. This contact is most probably frontal.

Daily flow charts over India north of 20°N. during the period Nov. to Apr. show a strong westerly at levels above 7,000 ft. This is part of the zonal northern hemisphere westerly which has split on the western side of the Asiatic highland massif, one branch continuing on the north side as Siberian westerlies, the other flowing over India. The latter is shown by the Indian Meteorological Department (1943) to be from a direction somewhat north of west over India with a maximum speed over Bengal which lies south of the central and widest part of the plateau. This southern branch has always been called the ' upper westerly ' at Hong Kong and the name is continued to differentiate it from the continental Siberian air to the north. The word ' upper ' was originally introduced since the flow is always observed over S. China as a current flowing above the winter NE. monsoon. It is a remarkably steady airflow, dry and comparatively warm, with its base marked off from the top of the monsoon air by a strong inversion and wind shear (often as much as 180 degrees) whose height averages about 10,000 ft in November and falls to about 6,000 ft in February (Heywood 1941).

South and south-east of the upper westerlies are trades. South of Japan they curve around the western side of the Pacific anticyclone and meet upper westerlies at the upper W. Pacific polar front which, it must be stressed, is not necessarily coincident with the W. Pacific polar front of the lower level. Particularly in the vicinity of S. China the former is remarkably steady in position whereas

the latter is constantly regenerated from the north. A belt of high pressure at 10,000 ft, normally consisting of separate shallow cells, extends westwards from the Pacific anticyclone across Siam and the N. Indian Ocean giving rise to the zonal branch of trades shown in Fig. 2. The high frequency of this flow is indicated in Fig. 3 which shows wind roses for approximately 10,000 ft at stations northwards from Singapore. At Bandon, trades are absolutely dominant but give way northwards to upper westerlies. The boundary between upper westerlies and trades over Indo-China and the Indian Ocean is thus seen as a series of weak anticyclonic eddies which may be considered as extending the upper W. Pacific polar front into the Indian Ocean, a system such as schematically described by Garbell (1947).

Fig. 3 shows that Singapore (Kallang) has predominant westerlies at 10,000 ft. These are the equatorial westerlies which are quite separate and distinct from upper westerlies. They are not weak, the highest frequency of velocities being greater than 10 kt, and the flow extends generally to the vicinity of Australia though, particularly in the area east of 120°E., it is often replaced by easterly currents and a convergence zone lying from north to south may then develop between the two flows. The discontinuity between zonal N. Pacific trades and equatorial westerlies is usually well-marked in about 3°N. and although not particularly active there is a marked change of weather across it from a more stable type in the north to an unstable type in the south. This feature is termed the *Equatorial – N. Pacific trades discontinuity*, and becomes extremely important later in the year when it is farther north.

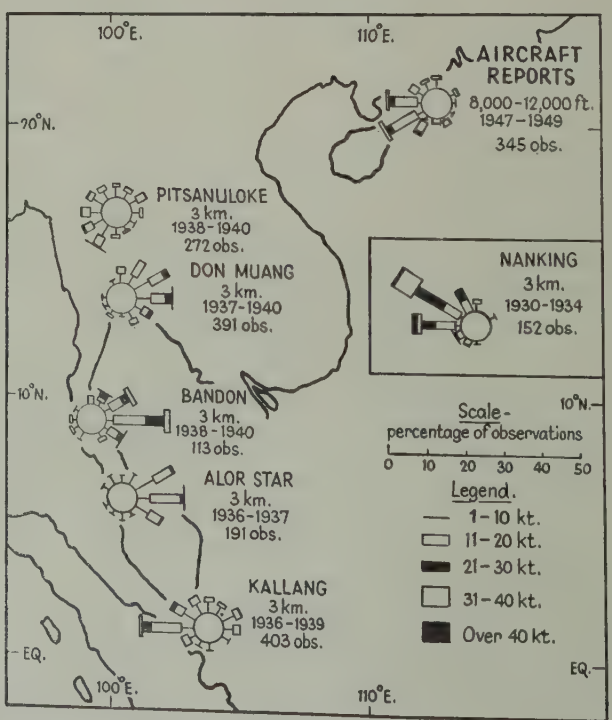


Figure 3. Wind roses for approximately the 10,000 ft level for the period Dec. to Feb.

(c) *The rhythm of the NE. monsoon*

Siberian anticyclonic (monsoon) surges are triggered by the development of face depressions off the continental sea-board between Formosa and Sakhalin amage 1948). The origin of these depressions is varied. Few depressions broaching the Siberian anticyclone from the west break through it, they are either flected round the north-east or they die out on the surface at its margin (U.S.W.B.) d proceed eastwards as troughs within the strong zonal current above the cold ock, developing downwards to the surface again on reaching the coast. Other pressions form as waves on the W. Pacific polar front or as lee depressions south Japan. Whatever their origin such depressions draw into their circulation cold of gathering momentum and, intensifying at rates varying very considerably, adually move away to the north-east. Fresh low-level Siberian air streams south-st and south under the influence of the deepening depression and the intensifying ticyclone until ultimately the depression is incorporated within the Aleutian low d an intensified and more extensive anticyclone has resulted. A steady state is ereupon reached and accelerative movement ceases, the W. Pacific polar front coming stationary. This is the beginning of a period of quiescence, and the ont eventually becomes diffuse.

It is rarely that a single trigger depression has so long a history and becomes vigorous that a surge extends far south. Monsoon air which is sometimes, though ry rarely, carried southwards as far as S. Indo-China is the result of two or even ree separate trigger depressions. Thus the first may extend the W. Pacific polar ont to central China and then, before the quiescent period is established, a new pression may rapidly develop and continue the movement of the front, or a new ie, to a more southerly location. Such is the normal sequence at low levels and an ample is given in Figs. 4, 6 and 8.

Weather associated with a monsoon surge varies very much; geographical cation, the depth of the cold air, the intensity and extent of the surge and exact sition of the high-pressure cell and trigger depression all play a part. It is con-nient however to associate low-level monsoon surges with two types of flow distri-ution at 10,000 ft over China and Manchuria. The upper-level flow of Fig. 2 rarely reaks down, though naturally there may be variation in the exact extents of the rflows and the positions of the convergence zones. The only major variation which as been observed arises when an anticyclone develops over the Tibetan plateau. his is not common and happens usually in the first half of the winter period. here results a complete breakdown of upper westerlies and a stream of northerly r over China on the east side of the anticyclone. Monsoon surges may be associated ith either of these two flow distributions at the 10,000 ft level. The former gives otentially cloudy conditions over China, the latter clear conditions. Typical appenings in these two cases are now described.

(d) *Monsoon surges associated with westerlies aloft over China*

Let an upper-level trough moving within the Siberian westerlies be destined develop to the surface and initiate a surge when it reaches the coast over the ellow Sea. In this position the winds at 10,000 ft over N. China will already be

largely NW. and the Tibetan lee convergence zone may accordingly have intensified, with the formation of medium cloud and perhaps some movement to the south. With the development of the low-level circulation over the Yellow Sea and the rapid deepening which almost invariably occurs, fresh Siberian air begins to stream southwards at low levels, the Siberian anticyclone intensifies rapidly, and a new polar front, separating fresh and stagnant Siberian air, comes into being and moves south-eastwards under the influence of the new depression.

The western part of the front, sweeping southwards, reaches its maximum intensity when it lies beneath and coincidental with the Tibetan lee convergence zone. Much precipitation may occur and there is a tendency in more stagnant situations for wave depressions to form, roughly along the line of the Yangtse Kiang. However, if the process goes on as normally, the polar front continues to move southwards beneath the upper westerlies, the Tibetan lee convergence zone remaining in the north. The NE. monsoon then appears as a slab of very cold air some 5,000 to 8,000 ft thick with its top marked by a strong inversion and an abrupt wind shear. Between Shanghai and Hong Kong the front becomes parallel with the isobars on the southern side of the anticyclone so that any further movement southwards depends upon the isallobaric tendency set up by the intensifying anticyclone, which often drives it beyond the coast of S. China. In this case the Siberian air, still remarkably cold, *cf.* Bannister (1948), converges strongly with trades flowing across the China Sea*. The final stage, when the front coincides in position with the upper W. Pacific polar front, is a gradual dying out due to high sea temperatures away from the coast and the moderating of the anticyclonic surge.

In the vicinity of Japan the W. Pacific polar front behaves in a different manner. It is little affected by the anticyclone and the upper W. Pacific polar front is here much more liable to movement than in the south. Accordingly the front can be very vigorous and typical cold frontal conditions usually occur.

The weather over China associated with a surge depends on the height of the inversion, the turbulence, and the moisture content, but the cloud development is always stratiform. Ramage (1949) has shown that a monsoon surge over Hong Kong does not produce appreciable rainfall ; indeed little more than drizzle occurs since the Siberian air is so shallow and is overlain by the strong inversion of dry upper westerlies. In the early part of winter the Siberian anticyclone, or a secondary cell, may move south to a centre over north or central China and then the air diverging from the anticyclone has little or no sea track by the time it reaches S. China and little cloud results. The latter half of the winter season however sees fewer digressions of the main anticyclonic centre, the air has a long sea track, and over China and the neighbouring seas the resulting weather may be low stratus and drizzle, often several hundreds of miles wide, extending back from the W. Pacific Polar front. When the fresh surge converges with trades off the coast of S. China the widespread stratus, drizzle, and blustery weather has become known as *crachin*.

Naturally a large number of complications or variations in this sequence may arise, as is only to be expected from a meteorological phenomenon covering so vast an area. Thus near Japan, Siberian air aloft may contact trades directly without

* It is customary to speak of the ocean between Formosa and Malaya as the China Sea, and that between China and the Ryukyu Islands chain as the Eastern Sea.

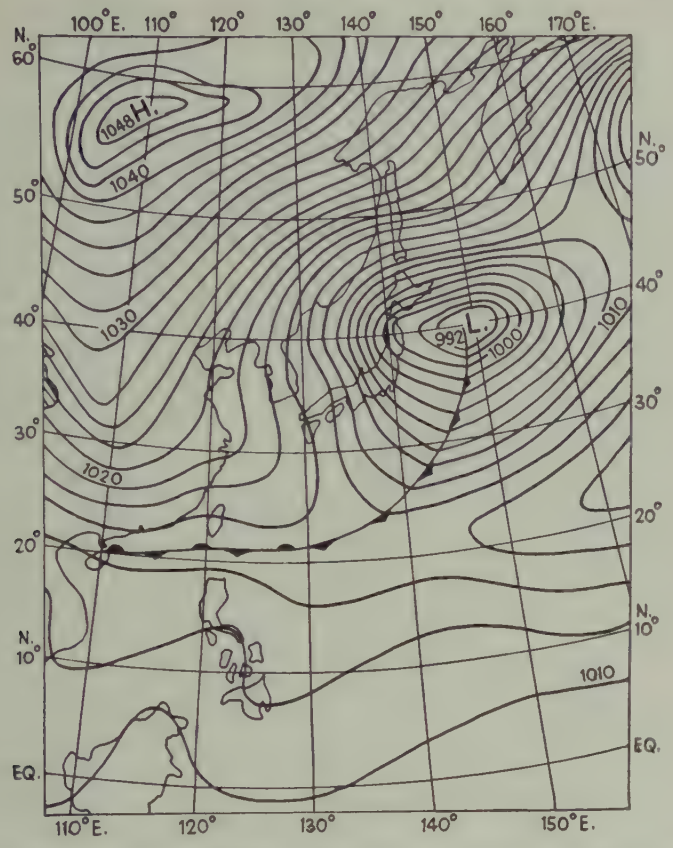
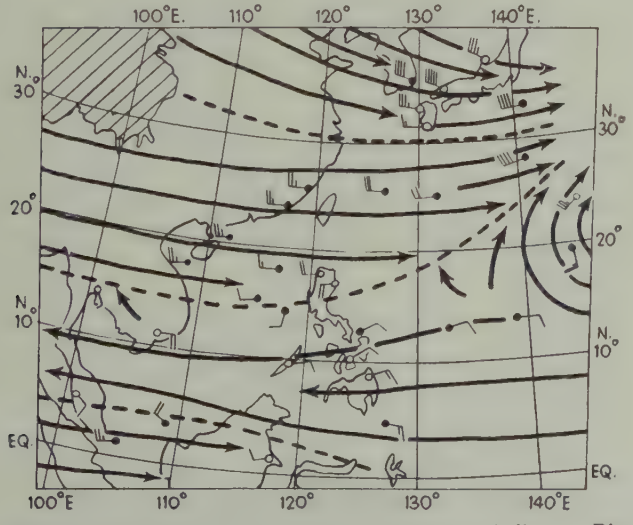


Figure 4. The synoptic chart following a fresh monsoon surge, 2 Jan. 1949 at 0000 GMT.



ure 5. Flow chart at 10,000 ft, 2 Jan. 1949. Open circles, pilot balloons. Black circles, aircraft. One full flèche represents 10 kt.

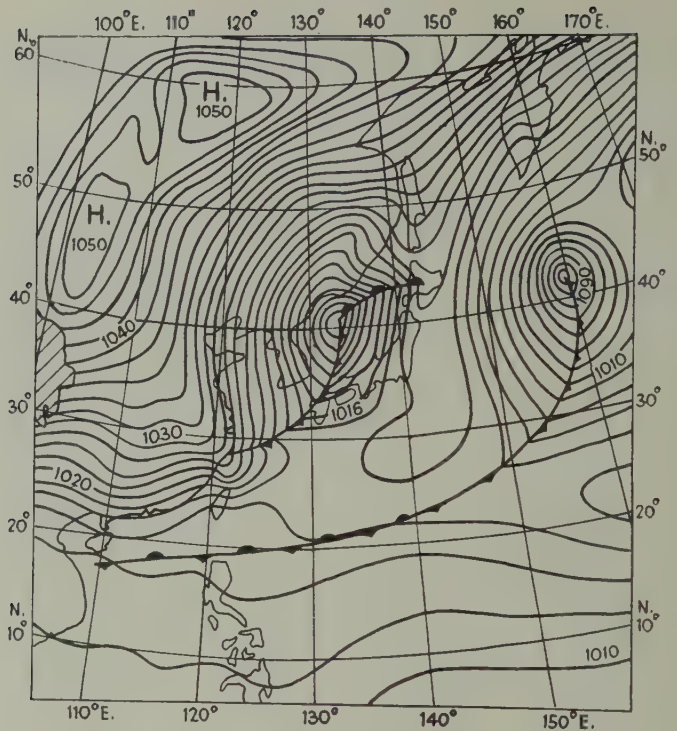


Figure 6. The initiation of a fresh surge of the NE. monsoon, 3 Jan. 1949 at 0000 GMT. (N.B. The isobar on latitude 40°, marked 1090, should read 1000).

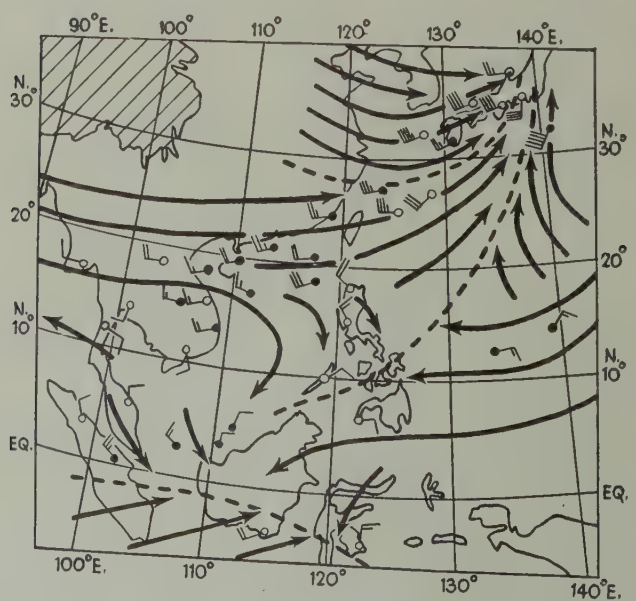
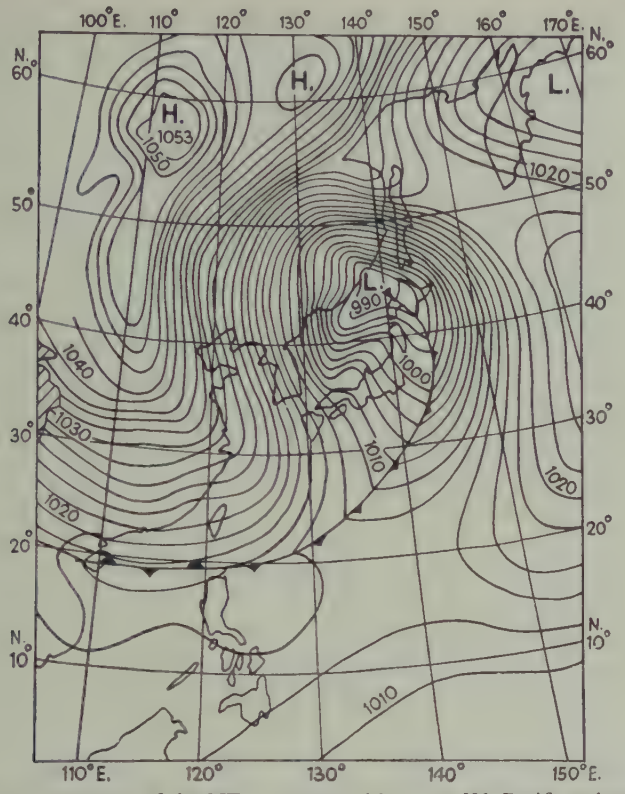


Figure 7. Flow chart at 10,000 ft, 3 Jan. 1949.



ıre 8. Vigorous new surge of the NE. monsoon with a new W. Pacific polar front, 4 Jan. 1949 at 0000 GMT.

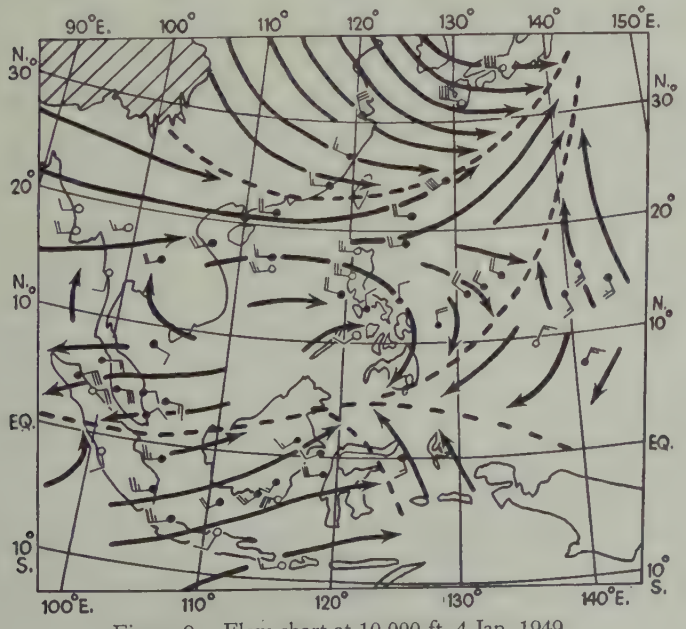


Figure 9. Flow chart at 10,000 ft, 4 Jan. 1949.

the interposition of upper westerlies, or the Tibetan lee convergence zone may be driven south-east beyond the coast of China, remaining coincident with the surface front, or even preceding it, for a long distance.

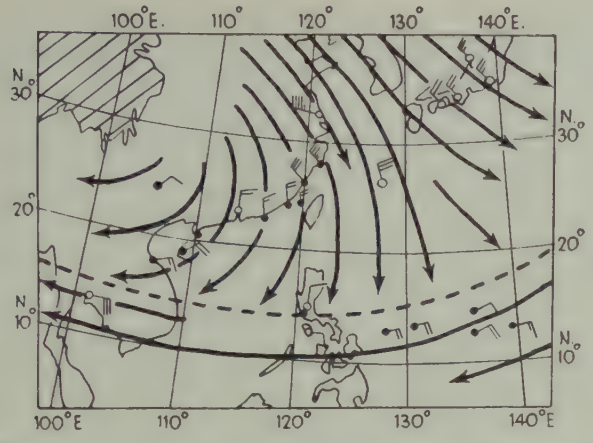
In Figs. 4 and 5 conditions are shown following a fresh monsoon surge. If the isobars of Fig. 4 are used as a rough indication of the lines of flow the correlation with Fig. 1 will be immediately evident. Similarly Fig. 5 is almost a replica of Fig. 2. From this stage the period of quiescence would normally be anticipated, the depression moving north-east towards the Aleutians, the anticyclone weakening, and the gradient slackening over Manchuria. A northward movement of the front towards S. China does not occur. Instead, as the depression moves away the sea track of the Siberian air becomes long and almost parallel to the trades, temperatures rise gradually over S. China, and the front dies out.

Six hours later a small circulation appeared on the surface chart over Korea. Its origin is uncertain since there was no earlier evidence for it, but by the next day it was an intense centre over the Sea of Japan, a fresh surge was sweeping southwards over China, and a new polar front had developed though the previous one had not yet disappeared (Fig. 6). Observe however from Fig. 7 that the winds aloft over S. China remained westerly and did not increase in velocity, nor were the upper winds over Japan increased from those of the previous day, though later they reached 70 kt in places. The upper W. Pacific polar front moved south-east over the Philippines in advance of the surface front and a convergence centre appeared to be developing at 10,000 ft over the equator. There was divergence from the area of southern Indo -China.

On the next day the surface chart, Fig. 8, was not greatly dissimilar from that of the first day except that the whole system was much more intense, but at 10,000 ft there had been considerable change. The Tibetan lee convergence zone had been driven to the coast of S. China and was strongly frontal, aircraft at 8,000 ft on the north side reporting temperatures of 1°C to 3°C, and others to the south at 9,000 ft reporting 9°C to 12°C. The annoying absence of information from N. Borneo precludes confirmation of a definite convergence centre in that area but there is a strong suggestion that it might have existed. The divergence area however was well-marked over the south of the China Sea, an unusual situation.

(e) *Monsoon surges associated with northerlies aloft over China*

In this case, which is much less frequent than the previous, the surface synoptic charts have a similar sequence throughout the course of events as before. Aloft however the presence of northerlies indicates the existence of an upper-level high-pressure centre located over Tibet. Usually the northerly stream turns anticyclonically over SW. China and reaches India, upper westerlies being completely blocked off from the circulation. A chart showing such a flow is given in Fig. 10, but an example of the surface chart is not presented. On this particular day, and in general, it closely resembles that of Fig. 4. In these conditions there is no strong inversion over China and little cloud results. Frequently the low-level isallobaric divergence from the anticyclone is so strong that surface winds make an angle of 90° with the isobars and reach 30 kt, streaming southwards with a wholly continental track. There follows brilliantly clear weather over practically all the country.



igure 10. The Tibetan anticyclone. Siberian air displacing upper westerlies over China. 2 Jan. 1948 at 10,000 ft.

) Troughs in the upper westerlies and activity of the upper W. Pacific polar front

An important feature of the upper westerlies is the presence of troughs and pper fronts moving from west to east within the stream. Such features are extremely hard to detect with the scanty information available since they are completely nsulated from the ground by the flow of the NE. monsoon beneath them. They annot be recognised by the synoptic details on the surface chart. The temperature, ew point and wind recorded there are those of the monsoon and they do not vary ith the passage of the upper systems. Usually the only trace is a belt of rain north o south over China but again this is often obscured by the presence of wide areas f drizzle or light rain from orographic or monsoon causes. In February 1949 however a case of a very strong upper feature occurred. It moved from India and crossed he Yunnan plateau and S. China. Westerlies reached down to the surface and the nonsoon was completely, though only temporarily, displaced. Ramage (1949) has nown that pressure troughs such as this are associated with depressions, either urface or upper, moving eastwards across Siberia or Mongolia, and such appeared o be the case on this occasion ; limited evidence suggests that it extended to at ast 40°N. and thus had probably crossed the whole Tibetan barrier from west o east. Apart from such big features smaller waves or upper fronts also exist. It notable that in March and April these systems are associated with instability. hus, from the weather notes of the Royal Observatory :

> At 2300 GMT, 29 March 1949, conditions 4/8 St at 2,500 ft and patches on the surrounding hills at 1,600 ft, with intermittent light drizzle and surface wind 100°, 8 kt. At 2325 a single clap of thunder heard followed almost immediately by the onset of heavy rain (large drops) which persisted until 2340. Reconnaisance revealed a belt of Cu stretching from north to south which moved slowly eastwards until it disappeared from sight. Unfortunately cloud obscured the lower part of the Cu and its base could not be estimated. The movement of the St cloud was checked by nephoscope and found to be 050°, its height being now about 2,000 ft.

hese features immensely complicate the art of forecasting because they are so ifficult to detect.

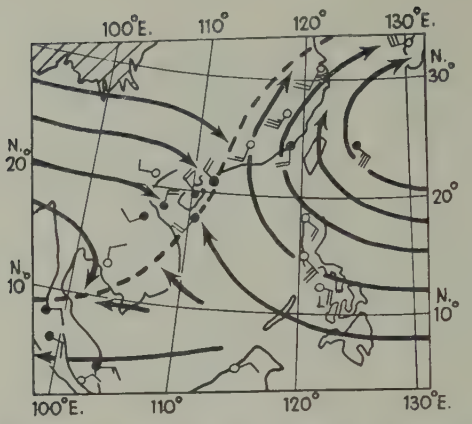


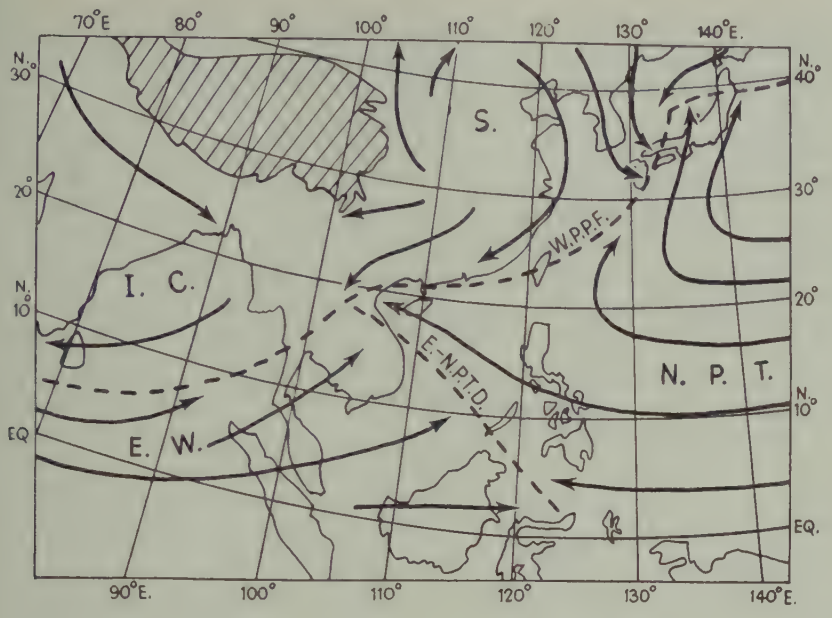
Figure 11. An active upper W. Pacific polar front resulting from the approach and incorporation of an upper front within the upper westerlies. 21 Jan. 1948 at 10,000 ft.

Fig. 11 shows the 10,000 ft flow associated with an upper feature which had moved eastwards within the upper westerlies. Trades had been drawn into the system in advance of the front so that it had become incorporated with the upper W. Pacific polar front and was strongly active. 30·7 mm of rain fell in the eleven hours the system affected Hong Kong, a considerable amount since the mean for January is only 31·8 mm.

4. May – the transition period

By May the pressure systems of the north are weakening rapidly and in the south new developments appear and intensify. Most obvious is the growing Indo-China thermal low, at first a weak and fluctuating feature which spreads gradually north and intensifies during the month. Across the N. Indian Ocean there develops the well-known equatorial trough and its so-called intertropical front. The Indo-China low is really a part of this trough but lying further north owing to the higher temperature of the peninsula than the seas to east and west. South-eastwards from the Indo-China low a similar trough sometimes develops but it is not common and usually there is little gradient over the China Sea. It is necessary to consider in more detail the structure of these three features, the equatorial trough of the Indian Ocean, the Indo-China low, and the infrequent China Sea trough. The main characteristics of the month however are light winds and shallow-pressure systems although, not to generalize too far, the SW. monsoon of the Indian Ocean can be strong, and tropical storms do at times develop there and within the China Sea.

With the development of the Indo-China low, Trades no longer penetrate into the Indian Ocean, though as seen from Figs. 12 and 13, they do occur at times as a weakening near-zonal flow across the north of the China Sea. Another effect is the gradual disappearance of upper westerlies from China, also attributable to the presence of the Indo-China low. The winter upper-level high-pressure belt from the Pacific to S. India is broken and two main cells become evident, one over the Pacific and the other, much weaker and dying, over S. India. Upper westerlies



igure 12. The airflows of May and their inter-relationship at 2,000 ft. Note how the equatorial trough of the Indian Ocean develops on the W.P.P.F. *cf.* Fig. 1.

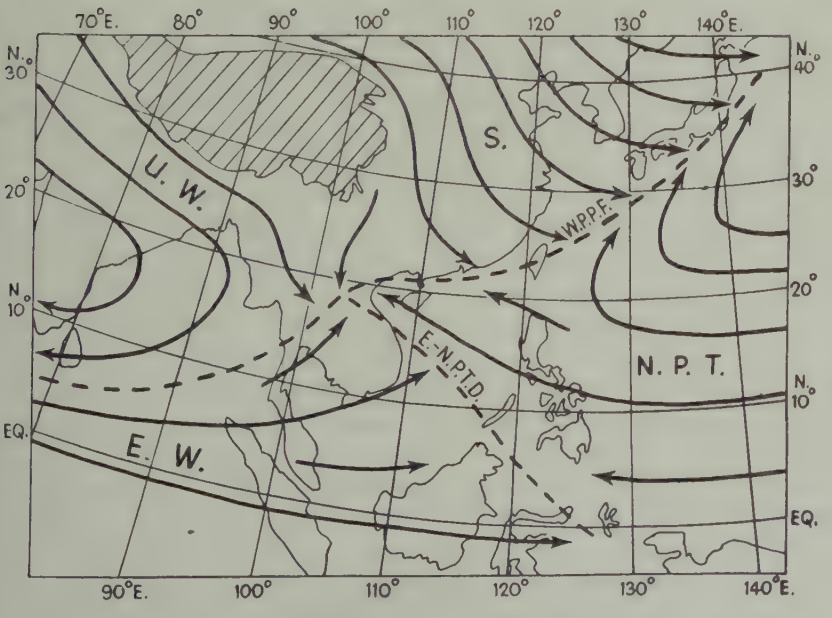


Figure 13. The airflows of May and their inter-relationship at 10,000 ft.

accordingly for a short space of time swing around this latter anticyclone to become easterlies on its southern side. With the absence of upper westerlies from China the Tibetan lee convergence zone disappears and Siberian air comes into contact directly with trades at 10,000 ft. The W. Pacific polar front, now having considerable depth, is immediately intensified.

South of the polar front over the Indian Ocean at 10,000 ft equatorial westerlies persist. At 2,000 ft westerlies or south-westerlies gradually develop. This is the SW. monsoon of the Indian Ocean, developing on the south side of the equatorial trough which, it will be appreciated from Figs. 12 and 13, is simply the W. Pacific polar front, a boundary having Siberian and Indian continental air on the north side and maritime trades or equatorial westerlies to the south. The origin of the SW. monsoon current on the south side of the front is open to some doubt. It has of course generally been assumed to originate in the southern hemisphere, largely because of records of surface winds. But these are backed from the 2,000 ft direction by friction, especially when they are light. In the writer's experience of that part of the Indian Ocean between Penang and Colombo, SW. surface winds are associated with westerly cloud drifts at 1,500 – 2,000 ft. Certainly over Malaya light south winds occasionally occur at 2,000 ft but hardly more frequently than light westerlies. With the very light winds near the equator it is almost certain that there cannot be sufficient direct transport of air from the southern hemisphere to feed the whole of the SW. monsoon, especially as winds are predominantly westerly at higher levels. It is suggested therefore that the monsoon current is largely a development from the equatorial westerlies observed all the year round at 10,000 ft. The monsoon current at 2,000 ft is regarded incidentally, not as spreading from west to east, but as developing more or less concurrently with equatorial regions. For instance these westerlies may appear, as can readily be envisaged from Fig. 12, in contact with Siberian air over S. China at the W. Pacific polar front, and heavy rain results before the same front – the so-called intertropical front – has reached Ceylon. The strength of this newly intensified polar front may be gauged from the increase of the five-day means of rainfall at Hong Kong as given by Jeffries (1939) for a 55-year period, *viz.* 1 to 5 April 3·0 mm, 1 to 5 May 5·6 mm, 31 May to 4 June 12·9 mm. Thus the summer monsoon rains start in Hong Kong (22° 18'N.), before the normal date of onset of the SW. monsoon (25 May) given by the Indian Meteorological Department (1943) for Ceylon, (below 10°N.). This earlier onset of monsoonal rains over China, and of course over Burma, than over India, is a result of the more northward development of the low over Indo-China than over the Indian Ocean.

In the early part of May, trades sometimes continue zonally across the Philippines towards the Indo-China low, contacting the developing equatorial westerlies at the equatorial – N. Pacific trades discontinuity, which extends south-eastwards across the China Sea. This appears on the synoptic charts as the China Sea trough. Although usually indistinct this system, also found at 10,000 ft, may give rise to the development of tropical storms within the China Sea. At this season cyclones also develop in the Indian Ocean, and it is interesting to note that in both cases the initial structure is a discontinuity having westerlies to the south and easterlies to the north, a system found by Thompson (1951) to be common as a background of the development of typhoons at other seasons.

In the north of the area sequences similar to those of winter occur, though in modified form. The Siberian anticyclone is weakening and an increasing succession of depressions or troughs move across Siberia separated by travelling highs. As the depressions reach the coast they intensify and a surge of cold air spreads southwards behind them towards Korea and China, their following ridge or anticyclone eventually spreading over China, and the W. Pacific polar front being revitalised. The next stage is the moving away of the depression to the north-east, whereupon the anticyclone over China merges with the Pacific high and gradually recedes eastwards, the polar front either dying out or moving northwards as trades or N. Indian westerlies set in across China in advance of the trough of the next Siberian depression. A new W. Pacific polar front is due to be formed and the procedure to repeat itself. As the season wears on such surges become weaker though they continue to reach Hong Kong at times.

5. June and July – the full summer monsoon

(a) *General*

By June the deep depression has developed over India and a trough – the polar trough – extends from it across Bengal and Yunnan to the former Indo-China low which has become deep, though still fluctuating, over S. China. From here the trough continues to the Japan area where there is normal depression activity.

South of the polar system the westerly monsoon current, called by Deppermann (1940), 'N. Indian westerlies,' has developed and become dominant. It is not easy in practice to distinguish between equatorial and N. Indian westerlies, but the definition adopted here is that equatorial westerlies are that part of the westerly flow of the Indian Ocean near the equator which maintains a zonal direction, whereas N. Indian westerlies are that part lying to their north which acquires a component of motion to the northward.

North of the trough over NW. India continental air persists during both June and July, as described for instance by Sawyer (1947), and over China Siberian air still lies to the north. The general structure is indicated in Figs. 14 and 15.

As the deep and humid N. Indian westerlies contact Siberian air the W. Pacific polar front intensifies immediately and China's summer rains begin. The polar front lies near the coast of S. China at first but moves northwards, with occasional retrogressions, as the month goes on. Siberian air is brought southwards by a mechanism similar to that of May but depressions crossing Siberia gradually become shallower. Waves form on the polar front and move along it to reach their maximum intensity near Japan. These waves are very shallow compared with those of higher latitudes but the rainfall is large, the thermal contrast between the air masses being considerable and the N. Indian westerlies very humid.

With the encroachment of N. Indian westerlies into the area between China and the Philippines the W. Pacific convergence zone is formed where the flow meets trades. Notice particularly from Figs. 14 and 15 its extension to the southern hemisphere, and that below the equator S. Pacific trades have developed and thrust westwards, the W. Pacific convergence zone separating them from equatorial Westerlies.

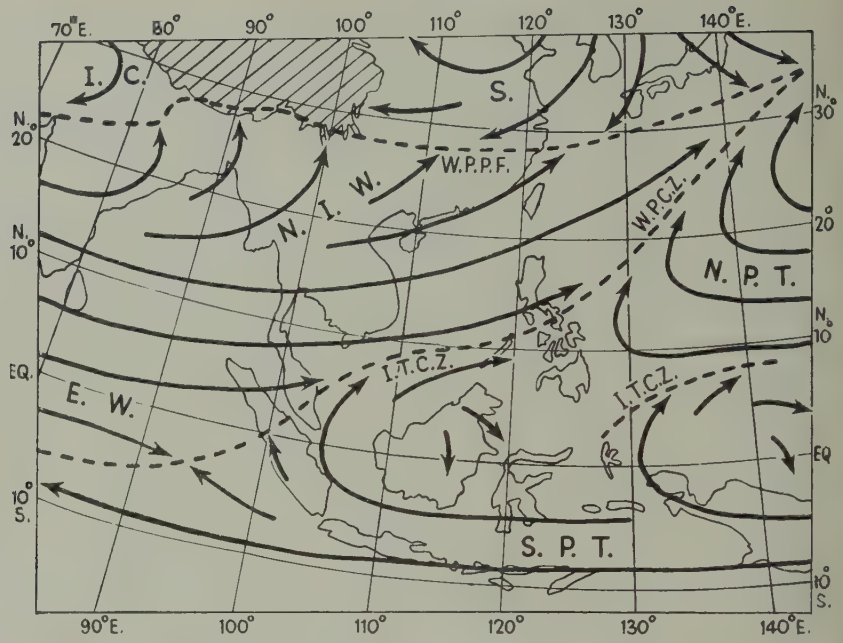


Figure 14. The general flow pattern in June at 2,000 ft.

N.I.W. – N. Indian westerlies, S.P.T. – S. Pacific trades, I.T.C.Z. – Intertropical convergence zone, W.P.C.Z. – W. Pacific convergence zone

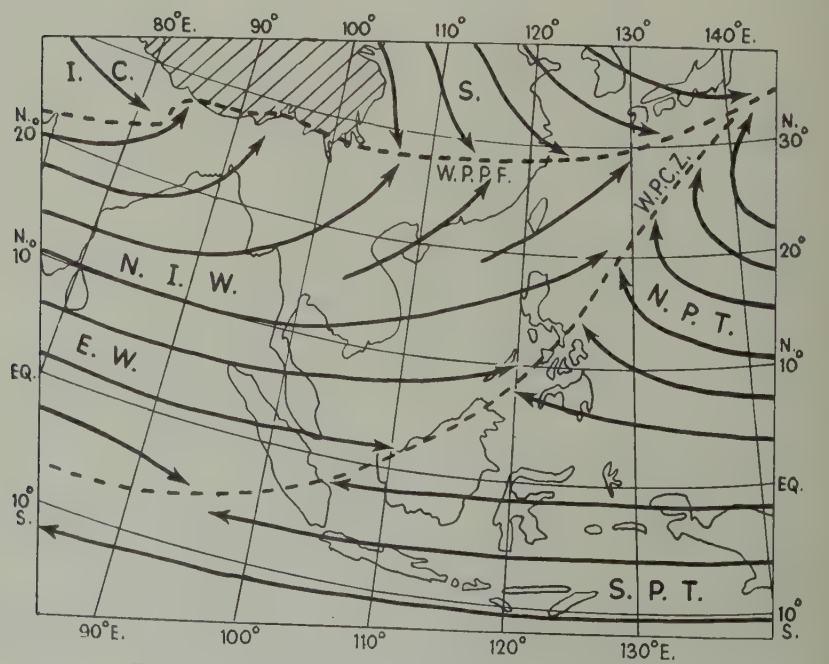


Figure 15. The general flow pattern in June at 10,000 ft.

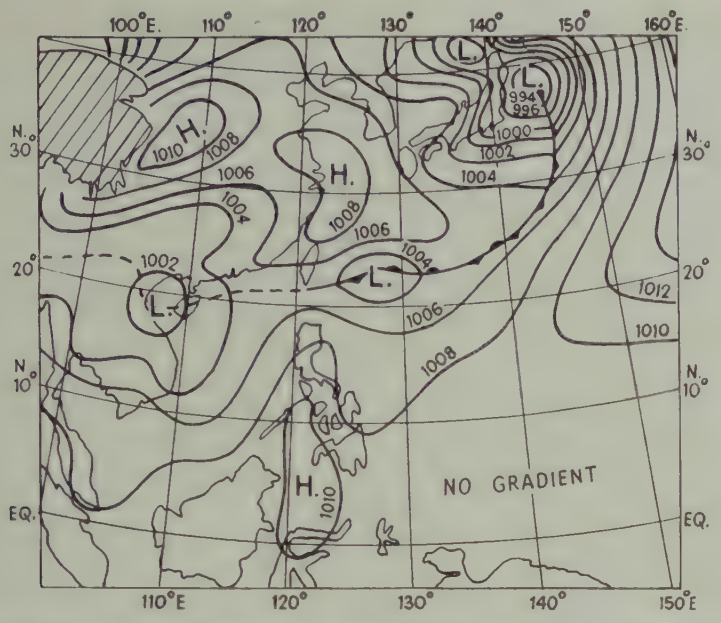


Figure 16. The W. Pacific polar front of summer. The China low is weak and centred over Tonkin. 3 June 1948 at 0600 GMT.

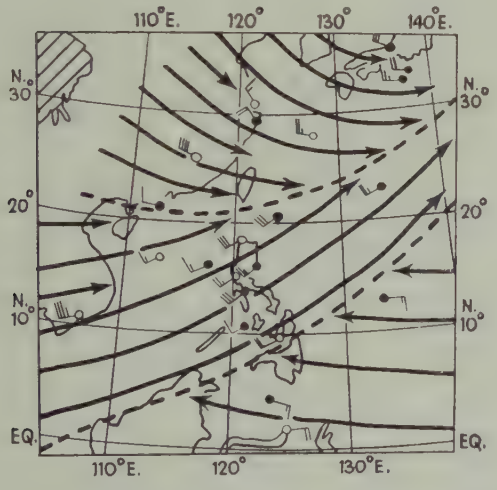


Figure 17. Flow chart at 10,000 ft associated with the surface chart of Fig. 16. 3 June 1948.

Figs. 16 and 17 exemplify a June situation. The surface chart, Fig. 16, is reminiscent of a typical winter chart. In fact it shows a summer surge of Siberian air, the polar front being south of Hong Kong. The China thermal low, sited over Tonkin, was unusually weak, whilst the equatorial trough extending the polar front towards India can be seen to the west of it. The airflow at 2,000 ft may be

gauged from the surface chart and can be compared directly with Fig. 14 if the isobars be used as rough indicators of the flowlines and if the eddy near Borneo in Fig. 14 be neglected. At 5,000 ft the airflow pattern was similar to that at 10,000 ft, Fig. 17, except that it was slightly weaker and that at Hong Kong a light easterly appears, showing an anticyclonic circulation to at least this height. Rainfall along the polar front was extensive, but there was little activity at the W. Pacific convergence zone. Charts nearly identical with this, except of course with the polar front gradually farther north as the month goes on, are repeated almost daily during June and could be illustrated here a hundredfold. The eastward extent of N. Indian westerlies to cover the Philippines is a noteworthy feature at this season ; one result of this is that most typhoons developing east of the Philippines have a northerly track in June and July, following the direction of the dominant airflow.

At this season the belt of N. Pacific trades begins to shift northwards and the doldrums form as a gradually widening zone between the trade belts of either hemisphere, eventually to reach a maximum width in September. Although the doldrums have come to be known by tradition as an area of light and variable winds the belt does show certain interesting characteristics. Thus in the early part of the season its light winds have mostly an easterly direction, probably due to the drag of trades on either side, but later westerly flows predominate. Occasionally eddies of S. Pacific trades spread across the doldrums belt, or equatorial westerlies may flood eastwards within it. A strong current of equatorial westerlies bounded north and south by equally strong trades, all three flows being zonal, is not uncommon over Borneo and may lead to very considerable weather disturbance over that country.

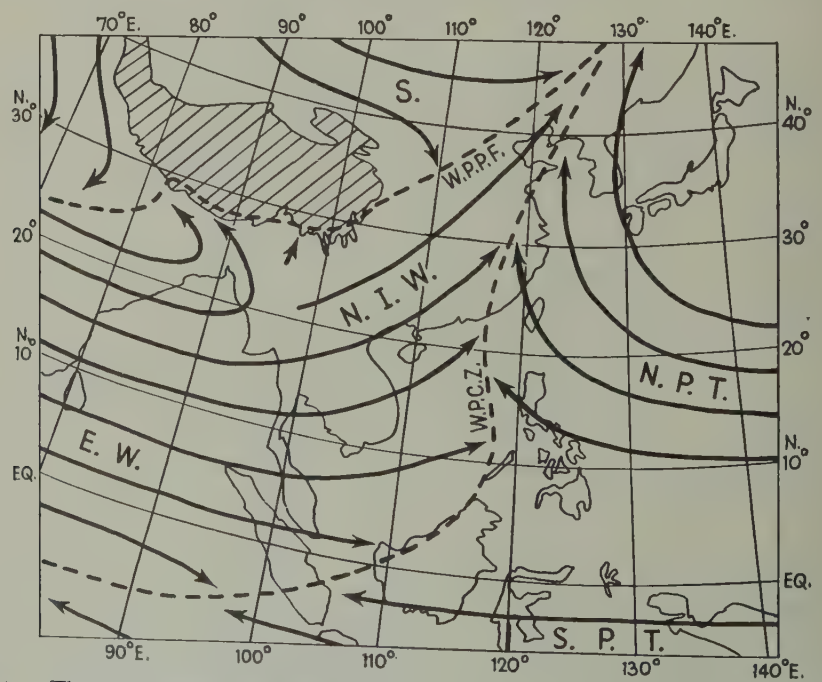


Figure 18. The most common flow pattern at 10,000 ft during late July. Flow at 2,000 ft is similar.

In the normal case the doldrums are reflected on the pressure chart as a zone of no gradient between the anticyclones of the two hemispheres. The effect of the recurrence of these westerly flows is the disappearance of the area of no gradient and its substitution by an active low-pressure trough marking trades to the north and the westerlies to the south. This is the equatorial trough of the Pacific.

By late July the China thermal low covers the north of that country, where it becomes a broader and less definite system than it was in the south during June. In accordance N. Indian westerlies have reached their farthest northward penetration. Over India the low is centred in the far north-west and there is a greater turning of air towards it from over Burma and the Bay of Bengal. The result is that N. Indian westerlies over China slowly become a weaker flow. Their direction is more southerly than during June, partly because of the new position of the China low, and partly because the Pacific anticyclone has moved north making the gradient more southerly over the Eastern Sea and Sea of Japan. It is notable that in July the relative increase in strength of the Pacific high allows trades to surge more and more frequently over E. China bringing rainfall to that country through the agency of W. Pacific convergence zone.

Fig. 18 is the typical flow pattern for late July showing the features described above. Only one model chart is presented since there is little difference between the flows at 2,000 and 10,000 ft.

(b) *The N. Indian Ocean westerlies and the equatorial westerlies*

N. Indian Ocean westerlies (briefly N. Indian westerlies) develop over the area from May on the southern side of the polar trough and their later strength is a direct result of the intensity of the thermal low-pressure systems built up over Asia during the summer. The strength and constancy of the flow during June and July are illustrated in Fig. 19.

The origin of the air has been discussed briefly under Section 4. The Indian Meteorological Department (1943) shows that between 0·5 and 4 km the majority of the monsoon flow is zonal westerly over India. Whether or not there is any turning of air from the southern hemisphere in the E. Indian Ocean is largely speculative, but Fig. 19 shows that there is little of such transport over Malaya. Winds over Singapore are predominantly westerly at 10,000 ft, indeed westerlies are just as frequent as S. Pacific Trades over S. Sumatra which is south of the equator. They lose their importance over Java but nevertheless they occur there quite frequently, often with a speed of 20 to 25 kt. There is reason to suppose therefore that westerlies predominate over and just south of the equator in the E. Indian Ocean as they do over Malaya and Sumatra. If this is so it is of interest to speculate how far the W. Pacific convergence zone of Figs. 14, 15, 17 and 18 extends westwards in the S. Indian Ocean, and whether or not there is ever activity upon it.

Much of the N. Indian westerly current undoubtedly originates in the southern hemisphere, crossing the equator in the W. Indian Ocean, but nevertheless a large portion is probably of northern hemisphere origin. Riehl (1948) says ' Doubt has also been expressed (Deppermann 1940) that all the air moving with a westerly component at low levels over the Indian Ocean is really of southern hemisphere

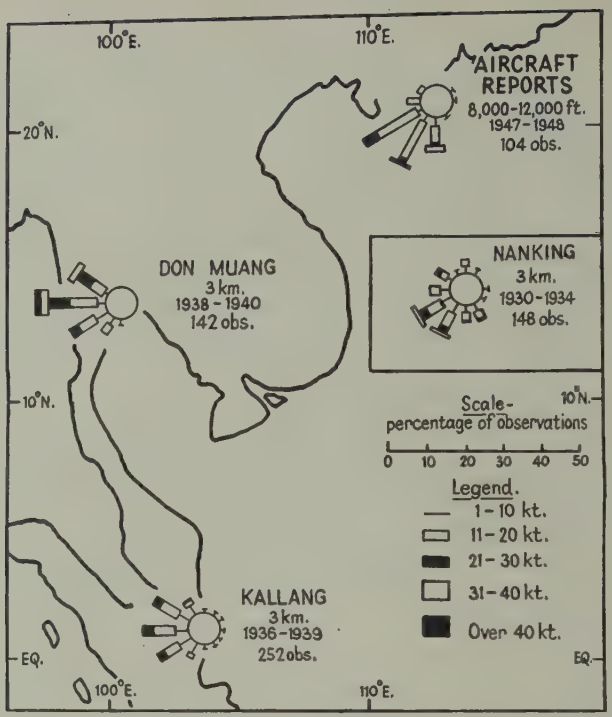


Figure 19. Wind roses for June and July at approximately the 10,000 ft level.

origin. In fact, flow charts prepared by Solot now prove that northern hemisphere air constitutes a large portion of this current.' But whatever their origin the N. Indian westerlies have had so long a history in the northern hemisphere before reaching the W. Pacific that there is no point in labelling them as southern hemisphere air over China.

Surges of N. Indian westerlies with velocity increases up to twice the normal 20 kt occur in the Burma – Siam coastal area and weather conditions deteriorate badly. Such surges gradually die out eastwards unless assisted by the coincidental occurrence of some feature east of the Annam Mountains, such as a tropical depression moving towards Hainan, towards the southern sector of which the flow converges, or the concurrent intensification of the China depression.

Conversation with aircrews flying the Hong Kong-to-Bangkok air route reveals the existence of narrow belts of altostratus and cumulonimbus cloud lying from north to south and unconnected with topography. Such features may well be ascribed to minor troughs within the airstream, few of which are revealed by the widely scattered synoptics of Indo-China and Yunnan.

The potentialities of the N. Indian westerlies as a rain-bearing wind are enormous. This is the monsoon current of the Burma coast and the chief cause of China's summer rains despite the föhn effect of the Yunnan plateau and the Annam mountains (this is the dry season for the Annam coast). It is evident that rain, other than local showers, will only fall given certain activating agencies, which are generally :

(i) Contact with Siberian air at the polar front.

(ii) Contact with trades at the W. Pacific convergence zone.

(iii) Contact with trades at the equatorial – N. Pacific trades discontinuity (see Section 6).

(iv) Velocity convergence or convergence due to cyclonic curvature which often occurs when the air is under the influence of a deep low over S. China, especially when it is centred over the Gulf of Tonkin.

The régime of the N. Indian westerlies may be briefly summarised :

(i) Rapid development in May over the Indian Ocean and Indo-China.

(ii) Advance across China, the Eastern Sea and into the Pacific, interposing itself between continental air and trades, during June and July, and reaching a maximum extent towards the end of the latter month.

(iii) A rapid dying out of the branch over China during early August and a spreading of equatorial westerlies eastwards into the Pacific doldrums during the same month (see Section 6).

(c) *Equatorial clockwise eddies and the intertropical convergence zone*

It has been shown earlier that the traditional intertropical front of India is in reality the polar front of Asia. Admittedly the air on the north side is not cold, nor of recent polar origin, but it is of high-latitude origin over both India and China ; having traversed a heated Asia and, in the case of NW. India, having experienced a recent föhn descent from the high plateaus of Iran, Arabia and Afghanistan, it could not possibly be cold. Petterssen (1940) and others show a polar front over N. America during the summer, but over Asia only a short feature in the extreme north-east. There is not this discrepancy between the two continents if the polar front is recognized in July over Asia as occupying the position shown in Fig. 18.

The accepted position of the intertropical front in July, which illustrated in Fig. 20, may be seen repeated in most meteorological, climatological or geographical literature dealing with the subject. It was arrived at by a study of surface winds over the area. The surface SW. monsoon of India, considered to be air of southern hemisphere origin, gave the impetus and it was found possible to extend the front across the Tibetan plateau to Manchuria because of the prevalence of southerly winds over SE. Asia. East of Manchuria the front was curved south to bring it to the position of the observed equatorial trough of the Pacific. During the northern winter it is observed that surface winds cross the equator into the southern hemisphere, so the intertropical front is then south of the equator. It was an easy step from this point to assume that the front moved north and south according to the season. The intertropical front is never observed on daily charts, nor does the annual movement occur. The use of climatological records of surface winds on which to base the traditional theory of the intertropical front was a very bad mistake. Tropical winds are notoriously fickle and light, and there is much danger in using light surface winds in meteorological analysis. It is fantastic that the theory of the intertropical front built on such a foundation can have so persisted.

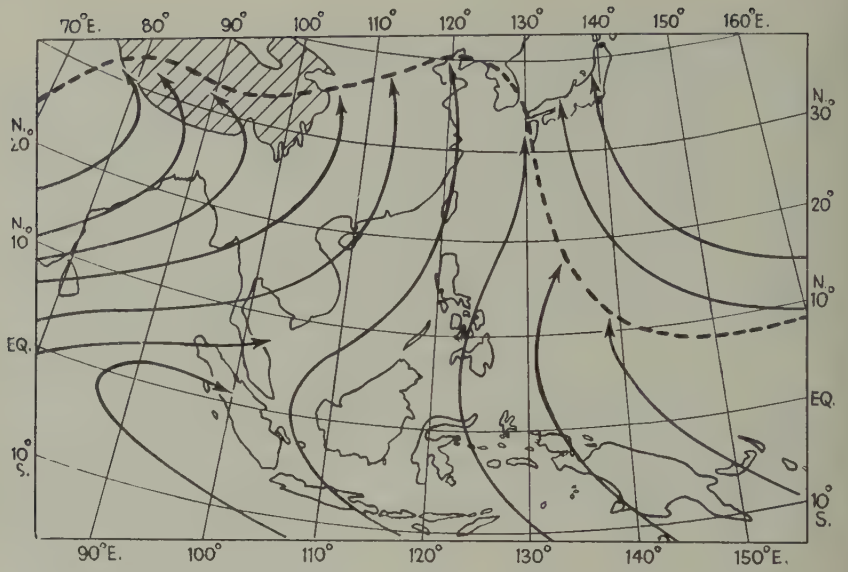


Figure 20. The classic picture of the intertropical front in July. An unfortunate myth.

Comparing Fig. 20 with Fig. 18 it will be seen that the traditional intertropical front is a fair approach to the limits of the N. Indian westerly current, which was assumed to be of southern hemisphere origin, and thus if it is to be accepted at all it should follow the line W. Pacific polar front – W. Pacific convergence zone.

There is no doubt however that the intertropical front is completely out of accord with the observed flow structure ; where for instance could the intertropical front be placed in Fig. 16 ? There is however a place for an intertropical convergence zone in this region during the summer but it is impermanent. It has always been observed that the intertropical front when in the vicinity of the equator is subject to irregular movement and considerable variations in intensity from day to day, very accurately described for instance by Wood (1947). So much is it the case that there would appear to be no permanent feature but rather something temporary and discontinuous which is observable on some occasions and not on others. This in turn implies that there is no smooth flow of air across the equator at any particular level.

Referring to the northern-hemisphere summer Riehl (1948) has recently suggested that within the equatorial trough across the Pacific there is no frontal system but instead a number of clockwise rotating eddies at low altitude. He says, ' such eddies may develop anywhere within the equatorial trough though the location north of New Guinea seems to be preferred.' Agreement would have been complete with this statement if the words ' equatorial regions ' had been substituted for ' equatorial trough.' Another favourite location for eddies is the region centring on Borneo or the Celebes Sea and an eddy here affects the China Sea where there is no equatorial trough in summer, pressure merging from high south of the equator to low over China. The structure of eddies is indicated diagramatically in Fig. 14.

A section of S. Pacific trades is diverted towards and across the equator whence it soon swings as an anticyclonically diverging flow towards the north-east and east. In the north-eastern sector of the eddy divergence is very marked, flow is weak, and part of the air turns back to the southern hemisphere. In the north-western sector air of the eddy converges with northern hemisphere air. This is the intertropical convergence zone.

Eddies occur most often at 2,000 ft, extend less often to 5,000 ft, and are rarely found at 10,000 ft where flows are most commonly zonal. They are irregular in occurrence, surge-like in character, and not simultaneous over a wide range of longitude. They persist usually from one to three days in the early summer and seem to attain a life of up to five days in August and September. One or two cases have been observed where the westerly winds on the north side of the eddy occur south of the equator, and hence Coriolis force is not an adequate explanation of the turning. A complex structure is hinted at and it is unfortunate that information from the southern hemisphere is not sufficient to study eddies in detail.

Eddies have been observed in winter in the New Guinea area when temporary easterlies were blowing south of the equator. West of this region however N. Pacific trades or equatorial westerlies prevail south of the equator and there is no opportunity for eddies.

It is seen then that there is not one uniform intertropical convergence zone but a series of impermanent convergence zones each of finite length. The western end appears towards the western side of the eddy, the eastern end dies out in the diverging air on that side. At any particular time two or more separate intertropical convergence zones may occur on the chart and the intensity of any one seems to depend of the following factors :

(i) The strength of the flow of northern-hemisphere air.
(ii) The duration and horizontal extent of the flow crossing the equator.
(iii) The vertical extent of the flow crossing the equator. Eddies are frequent at 2,000 ft but rare at 10,000 ft.

It is significant that aircrews often report an intertropical convergence zone but only rarely do they remark on excessive intensity – which will occur when the flow is deep. It will be appreciated that the convergence of only shallow layers of humid tropical air is sufficient to cause bad weather, and the deeper the airflows involved the worse and more extensive it is.

There are two main types of structure for this intertropical convergence zone, both exemplified in Fig. 14. Over the Pacific, eddy air converges with N. Pacific trades, an easterly current, the orientation of the resulting convergence zone being approximately west-south-west to east-north-east. It will be seen that in the northern part of the eddy the air pursues a track from the south-west or west through the doldrums before finally converging with the trades. The resulting flow discontinuity is reflected as a trough – the equatorial trough of the Pacific – and in this case the equatorial trough *is* an intertropical convergence zone. Such a situation is favourable for typhoon development (Thompson 1951). This is one of the two structures that the equatorial trough of the Pacific may have, the other is as the equatorial – N. Pacific trades discontinuity, described under Section 6.

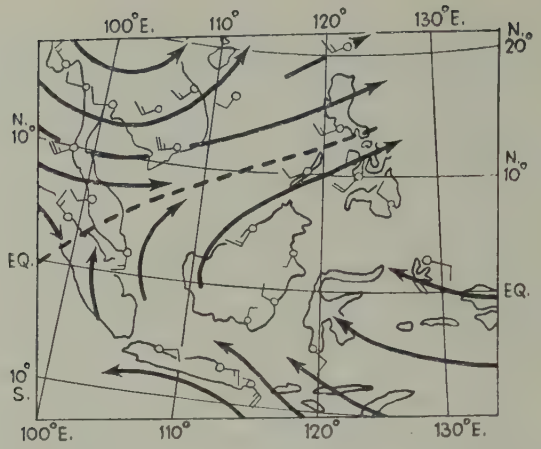


Figure 21. Violent intertropical convergence zone in the China Sea. The flow at 2,000 ft, 17 June 1949.

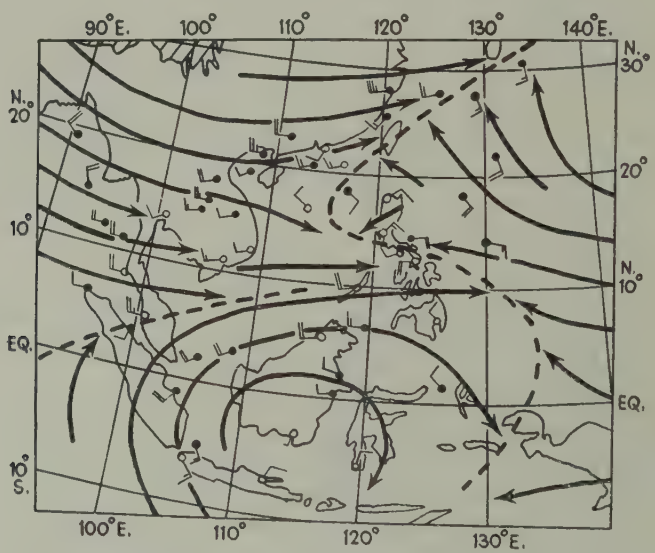


Figure 22. Violent intertropical convergence zone in the China Sea. The flow at 10,000 ft, 17 June 1949.

When the Borneo eddy develops, convergence is with N. Indian westerlies and in the mean the resulting convergence zone lies from Malaya to Palawan. This particular case is in reality an energized part of the W. Pacific convergence zone, and the difference from Pacific intertropical convergence zones is that the two contributory flows have approximately the same directions. Figs. 21-24 exemplify the Borneo eddy. Flow charts for a day when aircraft reported an exceedingly violent intertropical convergence zone between Singapore and Saigon are presented in Figs. 21 and 22. They show a deep eddy meeting N. Indian westerlies. It is

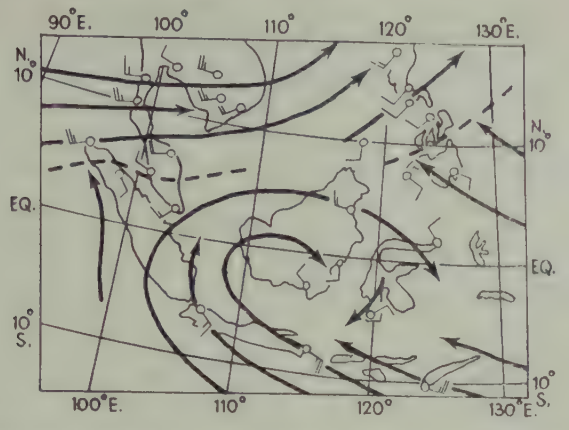


Figure 23. Clockwise eddy at 2,000 ft but with little convergence and no bad weather in the China Sea. 5 June 1949.

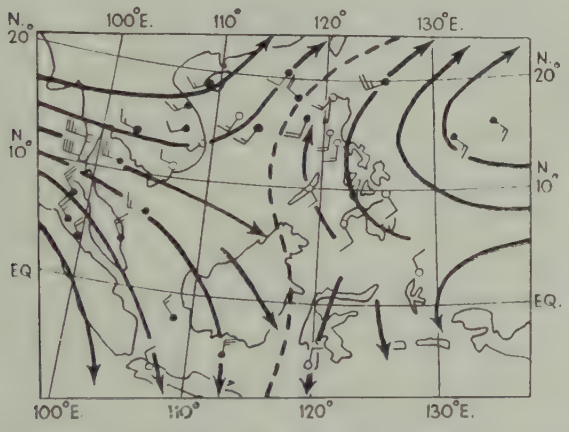


Figure 24. Flow at 10,000 ft, 5 June 1949. *Cf.*, Fig. 23, the same day but with an entirely different flow at the lower level.

remarked that tradition should have the intertropical front on 17 June much farther north than this and of very different orientation. As a contrast a weak intertropical convergence zone is shown in Figs. 23 and 24. This situation is much the more common. Flows at 2,000 ft and 10,000 ft are very different, only a shallow current crosses the equator, and no reports of bad weather were received.

This form of intertropical convergence zone occurs over the Pacific and China Sea, but from the Indian Ocean insufficient reports are received to demonstrate the existence of a similar phenomenon. It is not perhaps impossible that eddies develop at the discontinuity between equatorial westerlies and southern hemisphere trades which probably lies just south of the equator across the E. Indian Ocean in summer. There is no reason to assume that eddies do not occur over the Indian Ocean, the Borneo eddy being the easternmost of them.

6. Late August and September

The great thermal low of India is now weakening and N. Indian westerlies no longer reach China. Instead the Pacific anticyclone extends feebly over the centre of that country and there are shallow lows over S. China or Indo-China. Travelling depressions and anticyclones of Siberia are becoming more prominent, and more and more frequently anticyclones tend to lag in the Lake Baikal area.

This pressure pattern explains the main features of the model flow chart, Fig. 25. The usual mechanism brings air to the polar front which moves gradually south and reaches Hong Kong with considerable regularity during mid-October.

With the dying out of N. Indian westerlies, trades cover S. China, one branch turning north and north-east on the south side of the polar front, the other continuing west along the north side of the Indo-China low and reaching India. South of this zonal belt of trades, equatorial westerlies spread through the doldrums into the Pacific. The boundary between these two flows of opposite directions is the equatorial – N. Pacific trades discontinuity, now re-developed in stronger form after its almost complete eclipse during June and July. As mentioned earlier this is the second flow structure that the equatorial trough of the Pacific may have, and at this discontinuity the majority of Pacific typhoons develop. It is observed first in August as a more or less permanent system in the latitude of Formosa, and moves southwards with a regularity such that during the first two weeks of September it lies through the Luzon Straits to near Hainan, and the danger of a typhoon striking Hong Kong is very great during those two weeks. This discontinuity brings the last of S. China's summer rains. After its passage follows a period of trade-wind

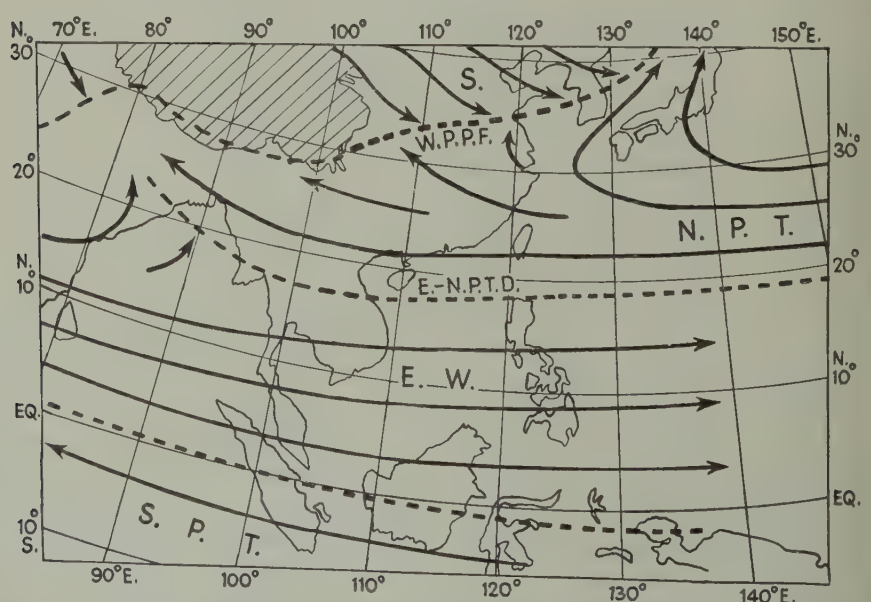


Figure 25. The common flow pattern at 10,000 ft in September. Flows at 2,000 ft are similar, though lighter except within the sphere of the Siberian air where they are, of course, liable to more variation than aloft.

ir and later Siberian air. The discontinuity continues to move south, causing rain nd tropical storms in the China Sea during October and November, but by the end f the latter month it has reached 3°N., its winter position shown in Fig. 2, and is oo far south for typhoons to develop.

Acknowledgments

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The north-east trade of the Pacific Ocean

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Abstract

First, the structure of the trade in the Pacific Ocean north-east of the Hawaiian Islands is shown with the use of vertical cross-sections, which extend to 3 km and include the fields of temperature, moisture, and motion. It is shown that the trade-wind inversion cannot be a discontinuity that separates an upper dry and lower moist layer. Downward transfer of mass through the inversion takes place.

Next, the balance of heat and moisture is calculated. Although the use of several assumptions and approximate procedures is involved, a rather satisfactory result is obtained. In accordance with previous studies, the net heat export appears to be in the form of latent heat.

The concluding section first offers a hypothesis concerning the destruction of the trade inversion. This is followed by a calculation of the momentum balance and a discussion of the mechanisms that distribute moisture and momentum in the vertical.

1. Introduction

Since the net radiational heat balance of the higher latitudes is negative and that of low latitudes positive, a poleward export of heat from the tropics takes place in the atmosphere that maintains approximate thermal equilibrium. The thermodynamic processes in the trade-wind belt are a principal link of this heat exchange. This fact, long recognized, has been the starting point for numerous studies enquiring into structure and thermodynamics of the trade region. Following the treatise of Sverdrup (1917) a group of German meteorologists led by von Ficker made detailed analyses of the observations collected in the Atlantic by the ' Meteor ' expedition (Kuhlbrodt 1928). Ficker (1936a) and Schnapauff (1937) described the fields of temperature, humidity and wind and their space derivatives as given by these data. Moreover, Ficker (1936b) calculated the heat balance in the trade-wind belt under certain assumptions. He reached the important conclusion that balance is maintained predominantly through the water-vapour cycle. Latent heat in the form of water vapour is *accumulated* by the trades. The release of this latent heat through condensation can take place in locations far removed from the site of accumulation. This theme has been developed further by Jacobs, who published charts of latent- and sensible-heat transfer from ocean to atmosphere in the North Atlantic and North Pacific Oceans (1942).

Subsequent to the ' Meteor ' expedition, special observations of a comparable nature were not gathered in the trade-wind region until 1945. Then two expeditions of the Woods Hole Oceanographic Institution visited the Caribbean under the direction of J. Wyman and A. H. Woodcock with the object to determine the structure of the trades below the trade-wind inversion. The principal results of these expeditions concerning heat transfer, to which frequent reference will be made, have been published by Bunker, Haurwitz, Malkus, Stommel (1949) (also unpublished MS. by the leaders of the expedition).

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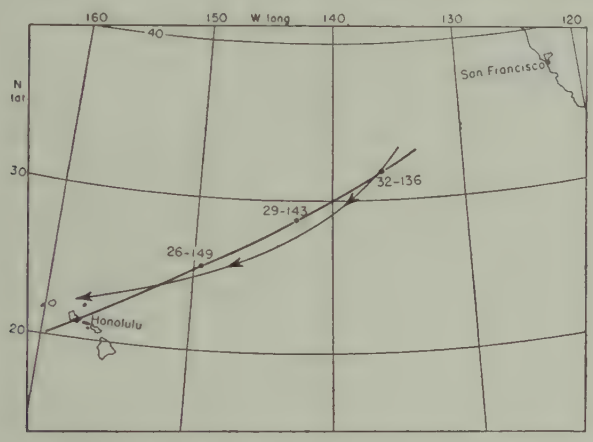
All of the investigations mentioned were handicapped to some extent by the ct that a sizable sample of homogeneous data along the air trajectory was not ailable. In this respect the regular observations taken by stationary weather ips of the U.S. Navy during the last war in the Pacific furnish valuable new aterial. A check with the monthly streamline charts published by Werenskiold 923) shows that during the months July to October (dry season) the average rface flow is almost exactly along the flight route from San Francisco to Honolulu, awaii, beginning at 130°-135° W. In this region, three weather ships were stationed 1945 so that together with the reports of Pearl Harbour and Hickam Field, both cated near Honolulu, four observation posts were located along the mean air jectory over a distance of 2,440 km (Fig. 1). These stations took hourly surface servations and two radiosonde observations per day. In addition, they took pilot-lloon observations which reached to 3 km but seldom much beyond that level.



ure 1. Location of weather ships July-October 1945, and mean surface air trajectory for period.

It was decided to analyse these data with a view toward extending the previous dies on the north-east trade. At first we shall show the structure of the trade in Pacific along the air trajectory and in the vertical. Then the balance of heat and isture will be calculated, followed by a discussion of the mechanisms for destruc-n of the trade inversion and of the vertical redistribution of heat and momentum.

The following symbols are used in the paper :

A = vertical turbulent exchange coefficient for water vapour.
E = evaporation from sea surface.
f = frictional force per unit volume.
F = vertical flux of latent heat.
I_0 = intensity of insolation at outer boundary of atmosphere.
k = coefficient of skin friction between ocean and air.
L = latent heat of condensation.
p = pressure.
P = precipitation.

q = specific humidity.
q_a = specific humidity at deck level.
q_s = saturation specific humidity.
q_w = specific humidity at sea surface.
Q_a = total heat exchange between ocean and atmosphere.
Q_e = heat exchange between ocean and atmosphere by evaporation.
Q_r = heat added to atmosphere by absorption of short-wave radiation.
Q_r' = total heat gained by air above clouds due to absorption of short-wave radiation.
Q_s = sensible-heat exchange between ocean and atmosphere.
r = radius of ascending convective element.
R = net loss of heat by radiation.
s = space co-ordinate along air trajectory.
S = wind steadiness (per cent).
t = time.
T = temperature.
T_a = air temperature at deck level.
T_w = temperature at sea surface.
u = total water vapour content above given level in atmosphere.
v = component of mean motion along s.
$\overline{\mathbf{V}}$ = vector mean (resultant) wind.
V_m = scalar mean wind speed.
w = vertical component of velocity of air.
z = vertical space co-ordinate.
α = albedo of clouds.
δ = declination of sun.
Δ = area element.
μ = vertical turbulent-exchange coefficient for momentum.
ρ = air density.
ρ_m = mean air density.
ρ_w = water-vapour density.
ρ_w^s = saturation water-vapour density.
σ = transmission coefficient of clouds for short-wave radiation.

2. Structure of the trade

(a) *Wind structure*

(i) *Resultant wind direction*

We begin by determining whether the air trajectory actually was along the great circle route from San Francisco to Hawaii during July to October 1945, the period analysed. For this purpose, monthly resultant wind vectors were calculated at intervals of 500 m from the surface to 3 km at each station for the observations taken at 1000 GMT and 2200 GMT. Unfortunately, the record at 26°N., 149°W. was quite incomplete, although synoptic map files show that the station was serviced throughout the period. Only two months of radiosonde data and less than one month of wind data are available.

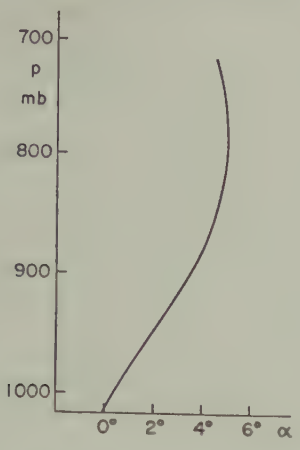


Figure 2. Average clockwise angular turning of wind with height, July-October 1945.

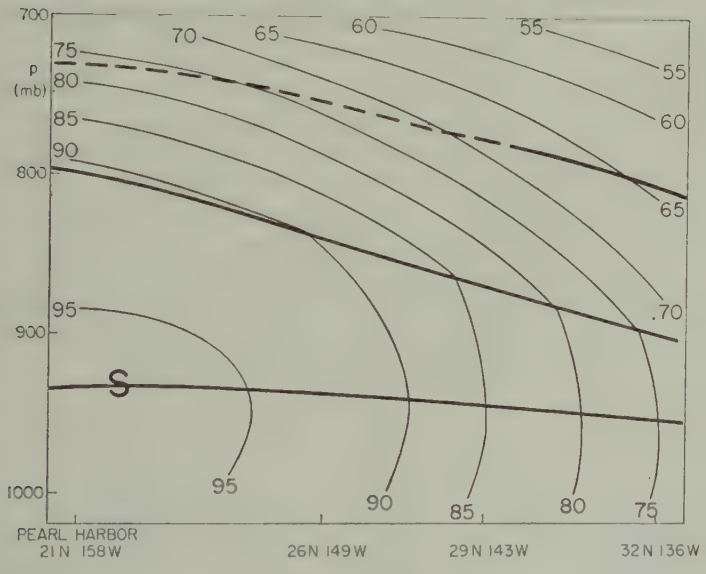


Figure 3. Vertical cross-section of wind steadiness (per cent). Heavy lines mark the boundary of the cloud layer, the base and top of the trade inversion.

The resultant wind vectors were sufficiently uniform from month to month, especially as to direction, to permit combination for the whole period. Fig. 1 shows the average trajectory for the whole layer from the surface to 3 km. A better coincidence with the great-circle route is hardly possible. Within the layer, the clockwise turning of wind with height was very small (Fig. 2) except at Honolulu where hills affect the surface wind.

(ii) *Wind steadiness*

The steadiness of the air flow (Fig. 3) was tested next. Steadiness (S), in per cent, is defined by the formula

$$S = \frac{|\overline{\mathbf{V}}|}{V_m} \times 100 \quad . \quad . \quad . \quad . \quad (1)$$

where $\overline{\mathbf{V}}$ is the resultant wind, and V_m the mean speed regardless of direction. Fig. 3 shows the distribution of wind steadiness in vertical cross-section. This figure also depicts the presence of four layers (*cf.* Fig. 8) : the subcloud layer extending from the surface to the bases of the cumuli ; the layer of trade cumuli which ends at the base of the trade-wind inversion ; the inversion layer ; and the air above the inversion top. (All the boundaries have been determined from the radiosonde data. The top of the subcloud layer was obtained by raising the surface air dry-adiabatically to condensation. As lifting and convective condensation levels are very nearly equal, this procedure is satisfactory within the limits of interest. Agreement between computed cloud bases and pilots' reports is good. The height of the inversion was determined by taking the mean of the daily heights at each station).

The distribution of wind steadiness is striking. Values are high everywhere and approach the maximum possible in the south-western portion. A general increase takes place downstream, as the air moves away from the influence of the aperiodic disturbances of middle latitudes. *Along the vertical the steadiness is uniform everywhere in the subcloud layer and decreases are small in the cloud layer. Above the inversion base the steadiness decreases rapidly with height.* The change in gradient is so pronounced that the isolines have been drawn with a first-order discontinuity at the inversion base. This perhaps is an exaggeration. But there can be no doubt that the field of motion changes its character rapidly as we pass upward beyond the moist layer in which vertical circulations predominate. *Synoptic disturbances are held to a minimum below the inversion base and increase rapidly in intensity upward above the base.* This is a characteristic of the wind structure of the trade region previously brought out and discussed by Riehl (1948, 1950).

(iii) *Wind speed*

Fig. 4 is a vertical cross-section of wind speed in the direction of the section, therefore very nearly of total wind speed at all heights. There is some doubt concerning the exactness of the lines near 26°N., 149°W. where, as stated before, only a fraction of the record is available. The following general features however are definitely established. Wind speeds are highest throughout the lower portion of the cloud layer. This agrees with the observation of Stone (1942) that the trade is strongest between 750 and 1,000 m at San Juan, Puerto Rico. Wind speeds decrease downward from this region in the layer of surface friction. They also decrease upward, and it is known from other studies that the easterlies disappear near 500 mb during the season in question. This decrease is found in corresponding locations all over the globe. Fig. 5(a) gives the vertical distribution of wind speed, averaged for the area as a whole. As a curiosity, the diurnal wind variations at 29°N., 143°W. and 32°N., 136°W. are added without discussion (Fig. 5(b)).

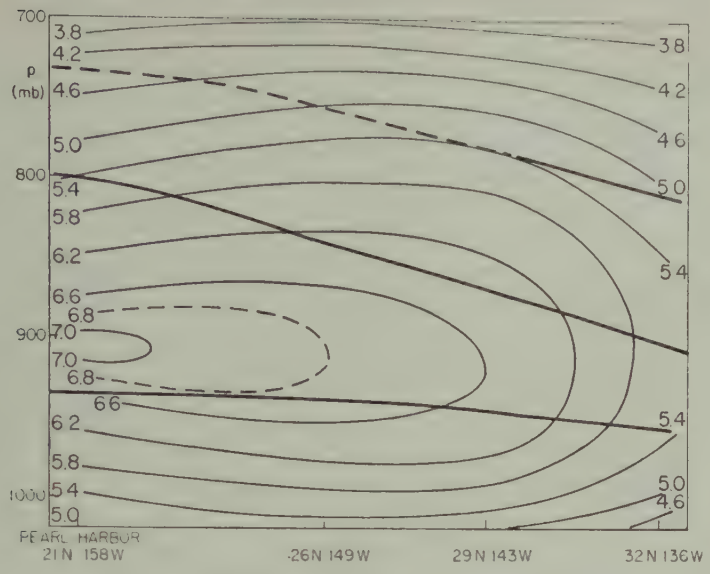
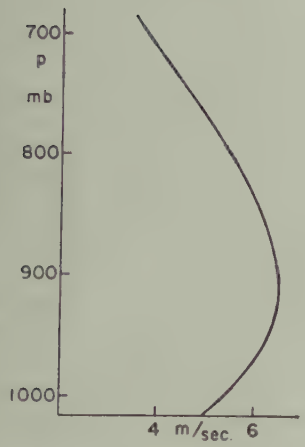


Figure 4. Vertical cross-section of wind speed (m sec^{-1}).



gure 5(a). Mean vertical distribution of wind speed.

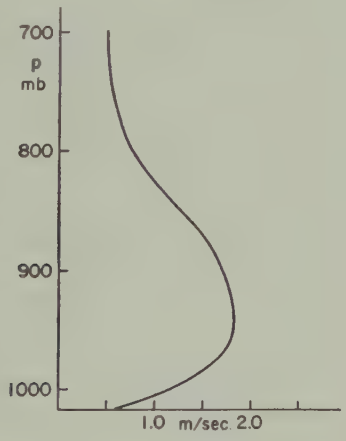


Figure 5(b). Vertical distribution of difference of wind speed 0100 minus 1300 local time at 32°N., 136°W. and 29°N., 143°W.

v) *Divergence and vertical motion*

It is possible to compute one component of the field of horizontal velocity and ass divergence from Fig. 4. *This component averaged over the whole section* (*Fig.* 6(*a*)) *positive throughout the entire layer considered, i.e., there is divergence. A level of on-divergence is situated at the very top near* 700 *mb.* We shall proceed under the sumption that Fig. 6(a) represents total velocity divergence. Our section lies ear the centre of the broad trade stream which has very little cross-stream variation direction over considerable distances at the surface (Werenskiold 1923 ; *Atlas*

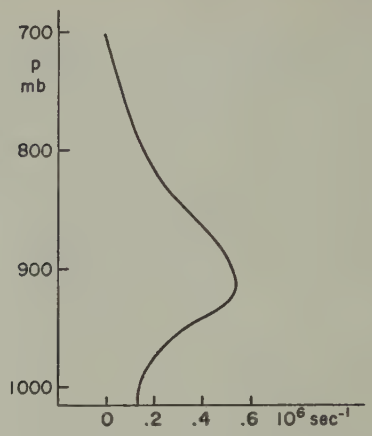


Figure 6(a). Vertical distribution of divergence (10^6 sec^{-1}).

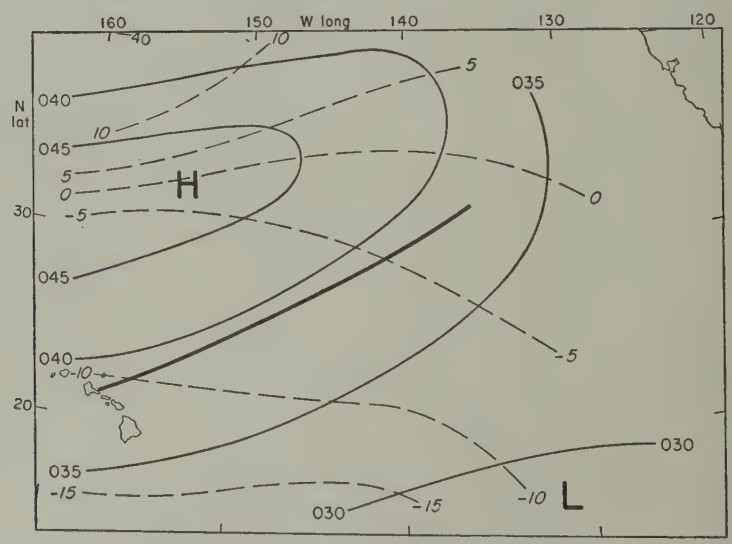


Figure 6(b). Mean 700-mb contours (10's ft, first digit omitted) July-October 1945, heavy, and lines of equal gradient wind speed (m sec^{-1} positive values have component toward east), dashed.

of climatic charts of the oceans, 1938). If anything, there is a slight fanning out downstream. The same may be deduced from the mean 700 mb-contour pattern for the period (Fig. 6(b)) for the upper part of the layer. Unless there is a much stronger cross-contour flow toward lower pressure just north of our section than to its south, it is safe to state that the contribution of the component of divergence normal to the section, if any, will be in the same sense as that of the component along the section. Previously Riehl (1950) had noted that there is surface divergence

in all oceans over most of the regions where the trade-wind inversion rises downstream, while Laskowski (1937) had computed divergence for the region of formation of the inversion.

The field of vertical motion can now be computed from the equation of continuity for the steady state,

$$\int_0^z \frac{\partial}{\partial z}(\rho w)\, dz = -\int_0^z \frac{\partial}{\partial s}(\rho v)\, dz \quad . \quad . \quad . \quad (2)$$

Here z is the vertical co-ordinate and ρ the density. As is to be expected from Fig. 6(a), the motion is downward at all heights (Fig. 7). The rate of sinking is quite appreciable in the upper parts of the layer. As the air needs 6 days to traverse the region, the net downward displacement from 32°N., 136°W. to Honolulu attains 400 m.

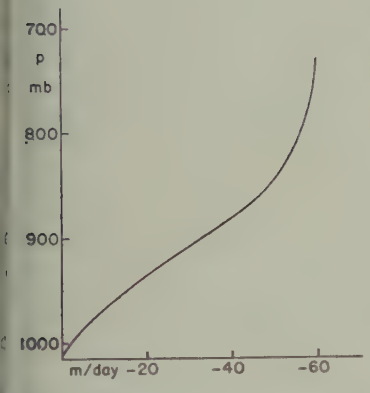


Figure 7. Vertical distribution of vertical motion (m day^{-1}).

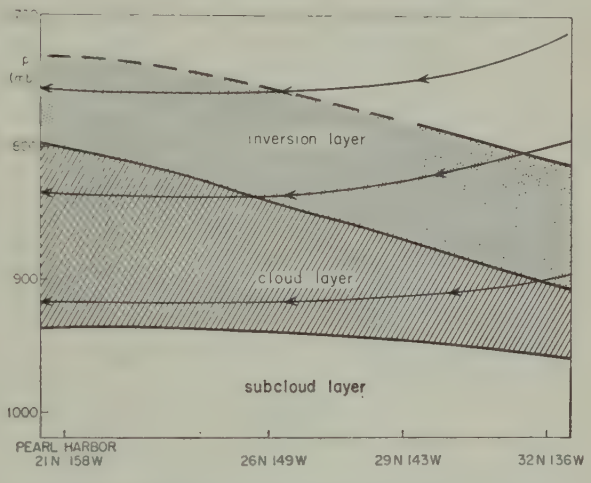


Figure 8. Illustrating the four layers of the trade-wind zone in relation to sample air trajectories.

The foregoing (*cf.* also Riehl and Yeh 1950) brings out clearly that it is necessary to re-examine previous descriptions of the rise of the trade inversion. *Whereas the inversion ascends downstream, individual columns descend, shrink vertically and spread horizontally* (*Fig.* 8). *Trajectories cross both inversion top and base. Large masses of air, located above the inversion at* 32°*N.*, 136°*W., have become a part of the cloud layer when they reach Honolulu.* If we consider the previous history of the air passing the Hawaiian islands, we can distinguish the following divisions (Fig. 9) :

(1) the air that has been below the inversion throughout the journey from 32°N., 136°W. ; (2) the air that has been incorporated in the cloud layer ; (3) the air that has been incorporated in the inversion layer. A later part of this report will describe a mechanism for the rise of the inversion against the divergence field.

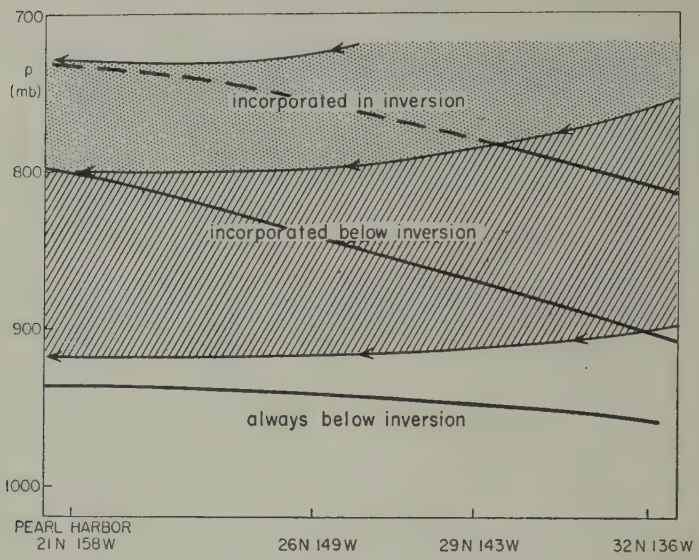


Figure 9. The flow of mass through the trade inversion.

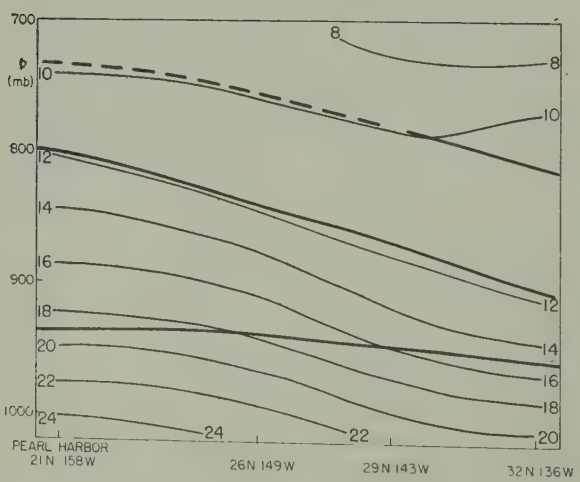


Figure 10(a). Vertical cross-section of the temperature distribution (°C).

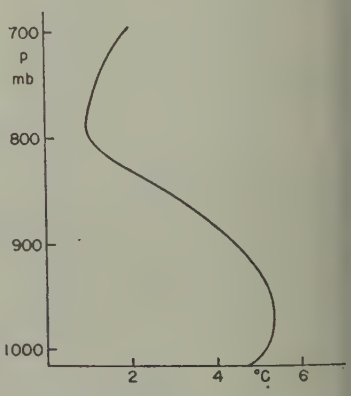


Figure 10(b). Vertical distribution of temperature difference (°C), Honolulu minus 32°N., 136°W.

(b) *Field of temperature and moisture*

(i) *Temperature*

Although two soundings were taken daily at all stations except 26°N., 149°W., only the night-time ascents (1500 GMT, 0300-0400 local time) were used to determine the temperature distribution. This restriction avoids the difficulties that

arise on account of diurnal temperature variations. Perhaps the most striking feature of the temperature field (Fig. 10(a)) is the fact that the isotherms run quite parallel to the trade inversion. The 12°C isotherm follows the base and the 10°C isotherm the top, as far as it is possible to identify the latter. A layer with an upward increase of temperature does not appear, but merely a deep isothermal stratum. Aperiodic variations in the height of the inversion are responsible for this spreading. Individual soundings indicate strong inversions up to 8°C, often contained within a shallow layer of as little as 20 mb. This is true especially at the north-eastern end of the section. There is one peculiar feature in Fig. 10(a), a downstream decrease of temperature between 750 and 800 mb from 32°N., 136°W. to 29°N., 143°W. No explanation is offered for this feature.

The total increase of temperature (Fig. 10(b)) from 32°N., 136°W. to Honolulu is greatest near the ground and then falls off to a minimum at 800 mb. This is reasonable since the top of the inversion is situated near this level at the north-eastern end, whereas the base of the inversion attains 800 mb at the south-western end.

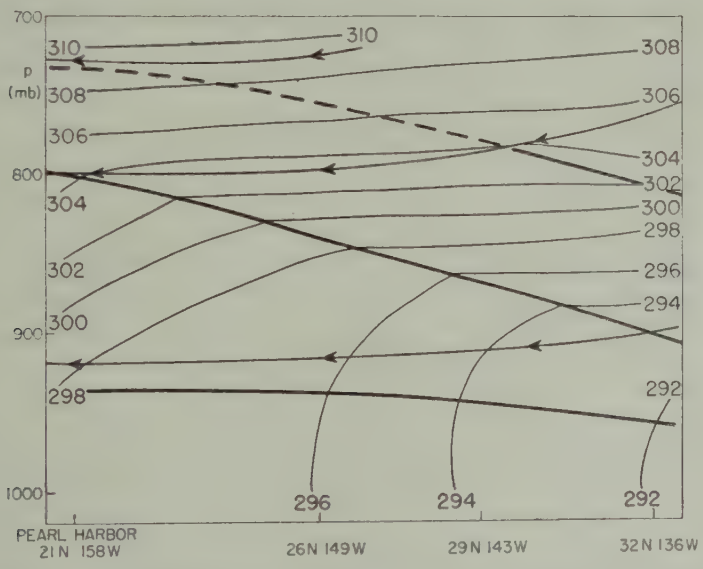


Figure 11. Vertical cross-section of potential temperature (°A) in relation to sample air trajectories.

The potential temperature field (Fig. 11) is in accord with that to be expected from the literature (Schnapauff 1937 ; Bunker *et al* 1949). Near the ground the potential temperature is almost constant with height. It increases slowly through the sub-cloud layer and somewhat more rapidly through the cloud layer. A first-order discontinuity marks the inversion base. Within the inversion layer, the potential temperature gradient is large but decreases slowly from north-eastern toward south-western end. In the cloud-layer, the reverse holds.

Many of the pertinent features of Figs. 10 and 11 become more evident when the temperature-lapse rate is drawn (Fig. 12). In the lowest levels the lapse rate is dry-adiabatic or greater, especially in the north-east. Near the cloud bases the

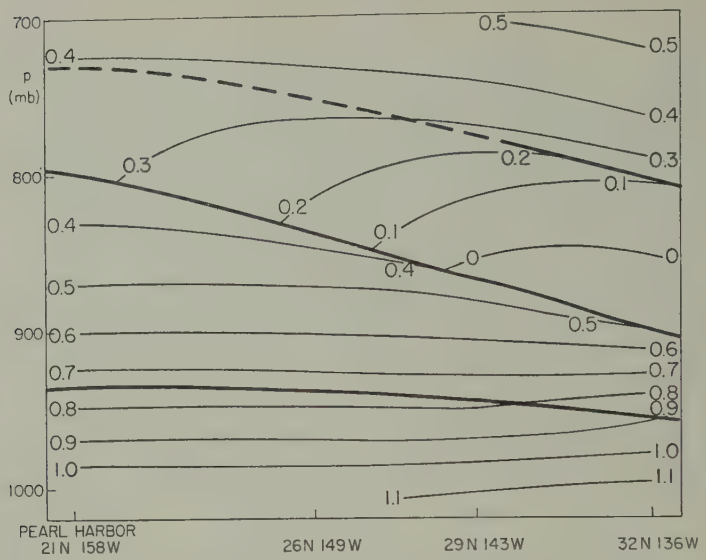


Figure 12. Vertical cross-section of temperature lapse rate (°C/100 m).

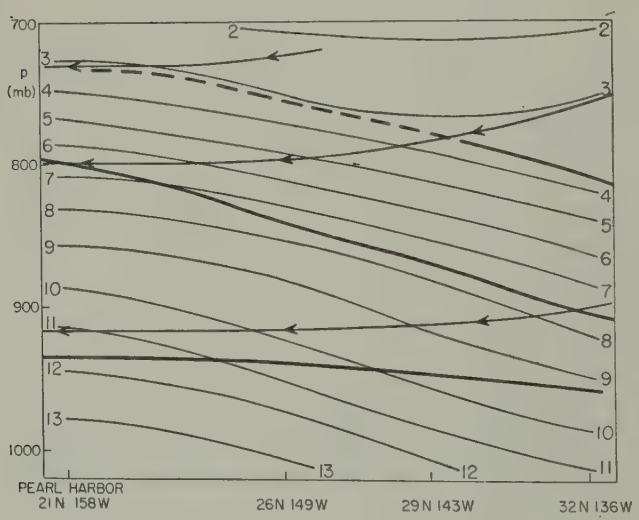


Figure 13(a). Vertical cross-section of specific humidity (g/kg) in relation to sample air trajectories.

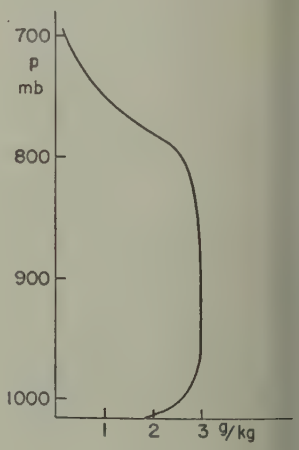


Figure 13(b). Vertical distribution of specific humidity difference (g/kg), Honolulu minus 32°N., 136°W.

stability increases quite rapidly, reminiscent of the secondary stable layer noted by Bunker *et al* (1949) on many of the ascents made near Puerto Rico. In the cloud layer the increase of stability again is more gradual. The lapse rate is discontinuous at inversion base and top, but has been drawn as a continuous line in those portions of the section where, in the mean, the upper boundary is marked merely by a gradual steepening of the lapse rate. A downstream weakening of the inversion is evident.

i) *Moisture*

As in case of the temperature, the lines of equal specific humidity (Fig. 13(a)) un parallel to the inversion. The motion of the air is toward higher moisture ontent everywhere. Of much interest is Fig. 13(b) which shows a nearly uniform ncrease of moisture up to 800 mb followed by a sharp drop. At the top of the section ae moisture increase nearly vanishes, demonstrating the rôle of the inversion in reventing much escape of moisture to the upper troposphere in the region analysed.

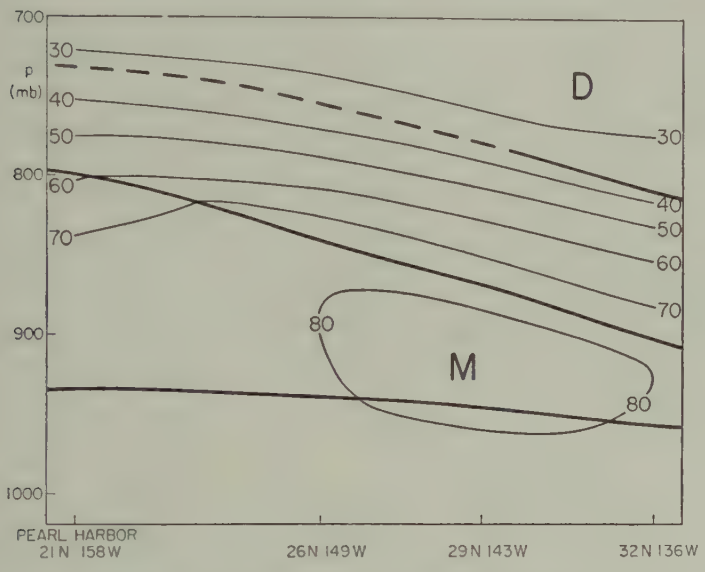


Figure 14. Vertical cross-section of relative humidity (per cent).

The relative humidity distribution (Fig. 14) is very satisfactory. This quantity s almost constant throughout the moist layer, with a slight maximum located near he cloud bases and in the lower part of the cloud layer. Relative humidity has a irst-order discontinuity at the inversion base, and almost the entire gradient is ontained within the inversion layer. In the highest levels the atmosphere is very lry as is to be expected.

A complicated pattern arises when we draw the moisture lapse rate (Fig. 15). The moisture decreases quickly with height in the lowest levels, in agreement with he findings of Schnapauff (1937). It is of interest, however, that the moisture-apse rate decreases downstream both in Schnapauff's data and in Fig. 15. For example, the total moisture decrease from sea-level to 960 mb is 2·6 g kg^{-1} at 32°N., 136°W., but only 1·5 g kg^{-1} at Honolulu. As the low-level air moves downstream, t gradually becomes more mixed.

Within the cloud layer, the moisture lapse rate reaches a definite minimum nd then increases slowly toward the inversion. It is a typical feature of many sound-ngs that the moisture begins to decrease fairly rapidly upward well below the nversion layer.

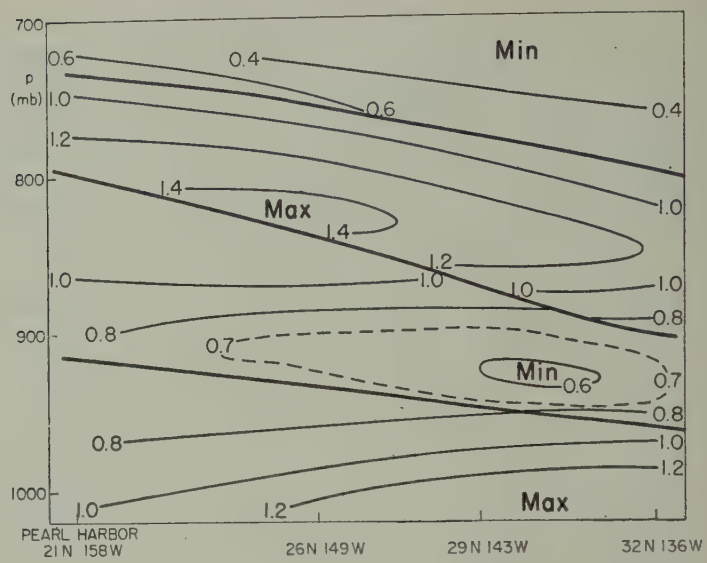


Figure 15. Vertical cross-section of moisture lapse rate (g kg^{-1} 250 m^{-1}).

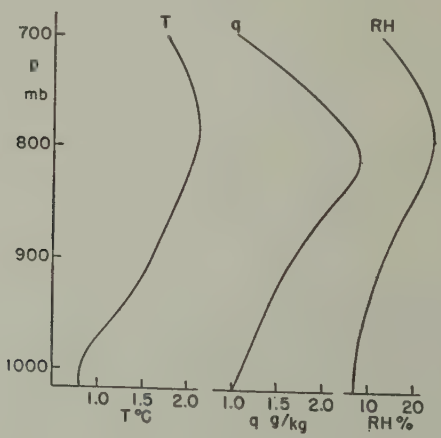


Figure 16. Average variability of temperature (°C), specific humidity (g/kg) and relative humidity (per cent) from day to day. This may not be a true inter-diurnal feature as pointed out in text.

A lapse-rate discontinuity marks both inversion base and top. Higher up, the lapse rate becomes very small as the moisture itself goes to low values. In the inversion layer, the largest moisture gradients are not situated near 32°N., 136°W. but in the centre of the section. This peculiarity finds an explanation in the fact that the aperiodic oscillations in the height of the inversion base are largest in the north-east, distributing the transition from moist to dry air mass over a deep layer.

Finally Fig. 16 shows the mean vertical distribution of the variation of temperature, specific humidity and relative humidity from day to day. All three curves have maxima near 800 mb, demonstrating that the variability is highest where the vertical gradients are largest. The potential temperature gradient determines the temperature fluctuations. To a considerable extent therefore the changes are due to variations in the inversion height as indicated before. Such variations can reflect the effect of changes in the synoptic situation or they may be produced by smaller-scale gradients along the inversion surface due to convection cells. If the latter, the fluctuations would not be truly interdiurnal in character. In order to decide this question it will be necessary to have a series of frequent (perhaps hourly) radiosonde or airplane traverses along the inversion.

3. The balance of latent and sensible heat

(a) *Transfer of heat from ocean to atmosphere*

The flux of sensible and latent heat from ocean to atmosphere can be calculated by two methods, both discussed by Jacobs (1942). One of these involves an estimate of the net radiation received by the ocean and does not make explicit use of the wind speed. The other method was developed by Montgomery (1948), based on the work of Rossby and Montgomery (1935). It essentially considers the rate at which water evaporated just above ocean surface is carried upward by turbulence. This approach appears more satisfactory since the speed of the mean motion as a measure of turbulence enters directly. It was adopted for purposes of this study. The sensible-heat transfer was calculated with the Bowen ratio (*cf.* Jacobs 1942) and checked with Montgomery's 1948 formula for computing the total energy exchange between ocean and atmosphere. The computations agreed to 10 per cent.

Following are the formulae used to obtain sensible, latent and total heat exchange between ocean and atmosphere (Q_s, Q_e and Q_a):

$$Q_s = 4{\cdot}16 \times 10^{-7} (T_w - T_a)\, v \quad . \quad . \quad . \quad (3)$$

$$Q_e = LE = 1{\cdot}71 \times 10^{-6} L\, (q_w - q_a)\, v \quad . \quad . \quad (4)$$

$$Q_a = Q_s + Q_e \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Here T_w, q_w are temperature and saturation specific humidity at the sea surface; T_a, q_a and v, temperature, specific humidity and wind speed at ship's deck-level; and the constants are determined following the methods given in the literature quoted.

Of the data necessary for calculation, T_a, q_a and v were given, but not T_w. In order to overcome this difficulty, the average temperature difference between ocean and air for July to October was obtained from the *Atlas of climatic charts of the Oceans* (1938) and it was assumed that this temperature difference held true for July to October 1945 (Fig. 17). The result was checked against the observations taken on the last cruise of the 'Carnegie' (1944). Agreement was almost perfect.

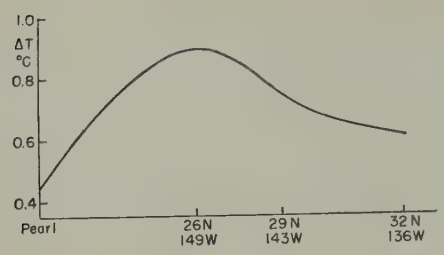


Figure 17. Temperature difference between ocean and atmosphere (°C).

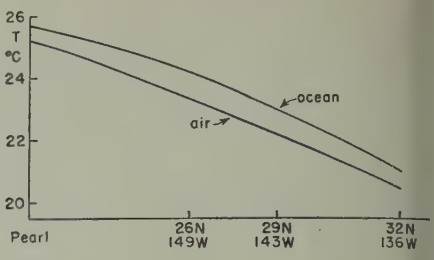


Figure 18. Air temperature at ship's deck-level and ocean temperature (°C).

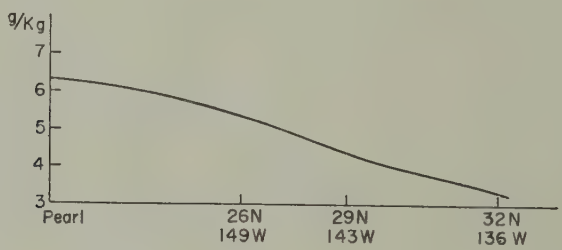


Figure 19. Specific humidity difference (g/kg) between the air at ship's deck-level and the ocean surface.

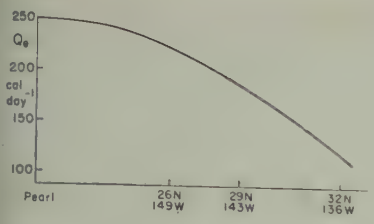
It is clear however that computations based on Eqs. (3) to (5) with the data mentioned can yield an order of magnitude only. The diurnal variations of T_a, T_w, q_a and q_w are all small, but the diurnal variations of $T_w - T_a$ and $q_w - q_a$ are large fractions of the mean gradients. It was not possible to consider these diurnal changes because T_w was not given. Finally the problem of determining the temperature at the very ocean surface is not entirely solved at present.

If we set these difficulties aside, all necessary quantities are easily obtained. Fig. 18 shows the air and ocean temperatures used ; Fig. 19, $q_w - q_a$; Fig. 20(a), Q_e ; Fig. 20(b), E in cm, and Fig. 20(c), Q_s. Integration over the whole distance yields $Q_e = 4{\cdot}9$ m cal/day (1 m cal $= 10^{10}$ cal) and $Q_s = 0{\cdot}4$ m cal/day. As noted in all previous studies, $Q_s \ll Q_e$.

(b) *Latent heat balance*

We shall now try to see whether it is possible to obtain a heat and moisture balance within the limits of accuracy of the computations. The equation of continuity for latent heat in a layer of unit thickness and cross-section and extending over the distance ds is

$$L\int \frac{\partial}{\partial s}(\rho q v)\,ds + L\int \frac{\partial}{\partial z}(\rho q w)\,ds + L\int \rho \frac{\partial}{\partial z}\left(\frac{A}{\rho}\frac{\partial q}{\partial z}\right)ds - L\int (E-P)\,ds = 0 \quad . \qquad (6)$$



igure 20(a). The transfer of latent heat from ocean to atmosphere (cal day^{-1} cm^{-2}).

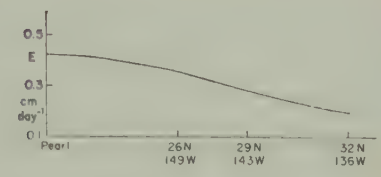


Figure 20(b). The evaporation from the ocean surface (cm day^{-1} cm^{-2}).

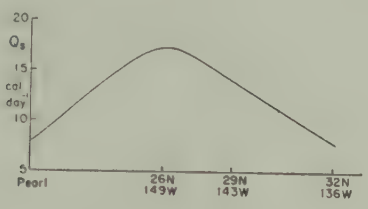


Figure 20(c). The transfer of sensible heat from ocean to atmosphere (cal day^{-1} cm^{-2}).

fere P is the precipitation and A the coefficient for turbulent exchange of moisture the vertical. In this form of the continuity equation, steady state is assumed and teral mixing is neglected compared to vertical mixing. Evidently, the second ssumption is justified, since the vertical stratification of the trade is highly un- able below the inversion, and since horizontal gradients of property are small. ig. 3 strongly supports the condition of steady state which certainly is more appli- ble here than almost anywhere else in the atmosphere.

As shown earlier (Fig. 9), we are dealing with three distinct bodies of air :) the air that is always below the inversion base ; (2) the air that is incorporated elow the base ; and (3) the air that is incorporated in the inversion. If we investi- ate the moisture balance in these three layers and attach the vertical co-ordinate the trajectory of the mean motion, w vanishes everywhere and the second term f Eq. (6) drops out, except at the top where the boundary is a horizontal surface. able I shows the calculated values of latent-heat transport (m cal/day) by the mean otion.

TABLE I

Layer	1	2	3	Total
horizontal inflow	3·2	1·9	0·3	5·4
vertical inflow	—	—	0·2	0·2
horizontal outflow	4·0	3·4	0·8	8·2
net outflow	0·8	1·5	0·3	2·6

These values are considered correct within 10 to 15 per cent. As the turbulent transport of moisture through the upper boundary is computed to be 0·5 m cal/day (see last sections of report), the total moisture export from the area is 3·1 m cal/day. The evaporation is 4·9 m cal/day and the precipitation therefore 1·8 m cal/day, corresponding to a raingauge measurement of 0·13 cm/day or 3·8 cm/month per unit area. It is not possible to check this result against actual rainfall measurements, except over the Hawaiian Islands where rainfall for the period was somewhat below average. Jacobs (unpublished manuscript) however has computed the mean seasonal precipitation over the oceans. His charts show that the average value for the season in question is 3·5 cm/month over the distance here investigated. Although rainfall fluctuates strongly from season to season (Riehl 1949), the good agreement of order of magnitude suggests that the calculated precipitation amount is entirely acceptable. The foregoing, therefore, is considered to be a good approximation to the total moisture budget. In order to complete the picture of the moisture flux within the lowest 3 km, it is necessary to distribute the precipitation over the three layers. This can be done only after computation of the sensible-heat balance.

(c) *Balance of sensible heat*

(i) The equation of continuity for sensible heat, analogous to that for moisture, contains horizontal and vertical accumulation through the mean motion and vertical turbulence. However, the term representing sources and sinks is replaced by $\int (R - LP - Q_s)\, ds$, where R is the radiation. Following are the calculated values for the sensible-heat flux through the mean motion (m cal/day).

TABLE II

Layer	1	2	3
horizontal inflow	39	42	13
vertical inflow	—	—	8
horizontal outflow	40	42	21
net outflow	1	—	—

This result is unfavourable as the order of magnitude of all terms is higher than in case of the moisture flux. Changes in the wind speed of only 0·2 to 0·3 m sec^{-1}, certainly within the range of error of the wind measurements, produce changes in the calculated sensible-heat flow equivalent to the total moisture flow, the evaporation, and also the radiation. Under these circumstances, it is not possible to calculate a complete balance. We can however try the continuity equation

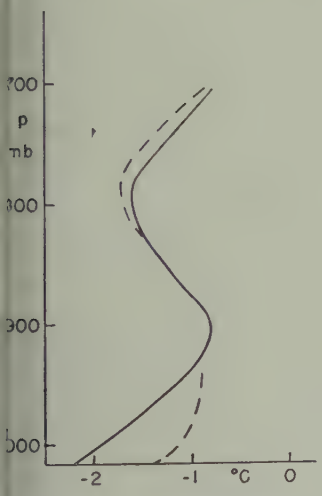
$$\int (R - LP - Q_s)\, ds = 0 \qquad . \qquad . \qquad . \qquad (7)$$

If this equation holds approximately then the net sensible-heat transfer is small and the trade mainly acts as an accumulator of latent heat, as suggested by Ficker (1936b). For evaluation of Eq. (7) we shall now compute the radiation.

i) *Long-wave radiation*

The calculations of the long-wave radiation were performed with use of Elsasser's 1940, 1942) radiation chart (second revised edition). Strictly, the cooling for each ay should be computed, and the daily values should then be averaged. In view f the small fluctuations of temperature and moisture, it was considered sufficient o make one computation at each of the four stations, utilizing the average tempera- ure and humidity distribution in the vertical. The solid curve of Fig. 21(a) shows he average daily cooling that would occur if the sky were entirely clear. A region f minimum cooling appears near 900 mb where the moisture-lapse rate is at a inimum (Fig. 15). The secondary maximum near 800 mb is due to the rapid ecrease of moisture with height above that level.

Inclusion of the cloudiness is a difficult problem since the cloudiness over the egion consists of scattered to broken cumulus. Elsasser has shown how to compute loud cooling if an overcast is present. Evidently we are here concerned with partial loudiness, therefore radiation from the sides of the cumuli as well as from the tops. No method has as yet been developed to include this feature. A second calculation f the long-wave radiation was made under the assumption that an overcast of the hickness of the cloud layer is present. The results for radiation with and without louds were then combined according to the ratio of cloudy to clear areas (mean loudiness). This ratio was obtained from Schott's *Geography of the Pacific and ndian Oceans* (1935) and also from the hourly synoptic reports for the period. It s of interest that the mean cloudiness decreases slightly down stream (From 6/10 o 4/10). As middle and high clouds are infrequent over the region in summer, he low-cloud cover may be considered equal to the total cloudiness with good pproximation.



igure 21(a). Vertical distri- ution of temperature change ue to long-wave radiation C day^{-1}) for clear skies (solid ne) and with allowance for ean cloudiness (dashed lines.)

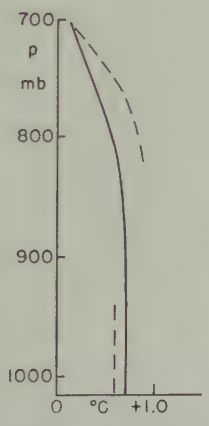


Figure 21(b). Absorption of short-wave radiation (°C day^{-1}) for clear skies (solid line) and with allowance for mean cloudiness (dashed lines).

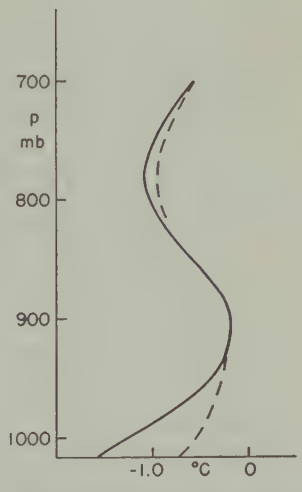


Figure 21(c). Vertical distribution of temperature change due to radiation (°C day^{-1}) for clear skies (solid line) and with allowance for mean cloudiness (dashed lines).

It is uncertain to what extent the outlined procedure approximates reality. But the correct order of magnitude should have been obtained. The dashed curve of Fig. 21(a) shows the effect of introducing the cloud cover. In the subcloud layer the cooling becomes much less, while above the clouds it increases slightly. It is difficult to distribute the cloud cooling reasonably over the cloud layer and the dashed line therefore has not been drawn for this layer. Introduction of the cloudiness effectively transfers the surface of greatest heat loss from sea level to the cloud tops. The net flux of heat into the cloud bases is small while the flux from the tops is very large. If the cooling due to the difference of flux across bases and tops is uniformly distributed throughout the cloud layer, the average cooling of this layer is about 3°C/day.

The total loss of heat from the section (including clouds) due to long-wave radiation is approximately 3·5 m cal/day, corresponding to a mean cooling of 2°C/day.

(iii) *Short-wave radiation*

The mechanism of absorption of insolation by the atmosphere is very complicated and an exact solution of the problem is still lacking. To date, Muegge and Moeller (1932) have given the most convenient method to calculate the absorption (Q_r) in a clear sky. Their formula is

$$Q_r = \frac{0{\cdot}0521}{u \exp 0{\cdot}6972} \rho_w \int (\sin \phi \sin \delta + \cos \phi \cos \delta \cos t)^{0{\cdot}6972}\, dt \quad . \qquad (8)$$

where u is the total water-vapour content above the level considered, ρ_w the density of water vapour at the level, ϕ the latitude and δ the declination of the sun. Q_r is the total daily absorption in cal/cm^3, if the integration is carried from sunrise to sunset. Eq. (8) shows that the absorption at any level is proportional to the vapour content at the level and inversely proportional to the total moisture content above the level. The moisture data given by our radiosonde observations only extend to 3 km. Above this level the total moisture content was calculated assuming a logarithmic decrease with height.

Fig. 21(b) shows the daily heating due to short-wave absorption in clear air summed over the section. The calculated values, 0·3 to 0·8°C/day, are in agreement with the computations made elsewhere (Moeller 1935 ; Tanck 1940). A space section of clear-air absorption (not reproduced) shows a maximum of absorption near the base of the trade inversion. Here the vapour content is still large but decreasing rapidly upward so that u is small above the inversion base.

The calculation of absorption of solar radiation on cloudy days, treated theoretically by Baur and Phillips (1934), is considerably more complicated. Sun beams striking clouds are in part reflected, in part transmitted and in part absorbed. The air above the clouds partly absorbs the reflected beams. If α is the cloud albedo, the intensity of radiation available for absorption above the clouds will be $(1 + \alpha)$ times that available in clear air. Below the cloud bases only the radiation that is transmitted through the clouds can be absorbed. But we must take into account that the radiation becomes diffuse after traversing the cloud. The absorption of diffuse radiation is twice that of parallel beams. If σ is the coefficient of transmission, multiplication of Eq. (8) with 2σ gives Q_r below the cloud bases.

Finally, the absorption inside the cloud is $(1 - \alpha - \sigma)(I_0 - Q_r')$ where I_0 s the insolation at the boundary of the atmosphere and Q_r' the total radiation bsorbed by the atmosphere above the clouds. As in case of the long-wave radiation, he computations for clear and cloudy skies are added together in proportion to the loudiness in order to obtain the net absorption.

Evaluation of the absorption for cloudy skies requires knowledge of α and σ. It is very difficult to obtain an accurate estimate for these quantities. Usually extbooks quote $\alpha = 0{\cdot}78$. However, as early as 1905, Schuster had shown theoretically that the albedo depends on cloud thickness. Recently Neiburger (1949) measured the reflection, absorption and transmission of stratus clouds in California. We have evaluated the absorption in cloudy skies with use of his experimental urves. This procedure is approximate in that Neiburger obtained his data from tratus clouds, whereas we are concerned with trade cumuli. Presumably the liquid-vater content of the trade cumulus is somewhat higher than that of stratus. Reflec-ion and absorption depend a great deal on the amount of liquid water. Since owever we are interested only in approximate values the procedure is justifiable.

The dashed curve of Fig. 21(b) shows the modification of the clear-air absorption esulting from introduction of the cloudiness. Above the cloud layer the heating s increased because of absorption of reflected radiation. Below the cloud bases t is decreased. The total absorption is 1·2 m cal/day, slightly more than a third of oss due to long-wave radiation. This is an appreciable amount which certainly annot be neglected as is done at times.

Fig. 21(c) shows the net radiation with and without cloudiness as a function of eight and Fig. 22 gives the spatial distribution of net radiational cooling in °C/day bove and below the cloud layer. It is seen that the radiational heat loss approaches ero under the cloud bases at the north-eastern end of the section.

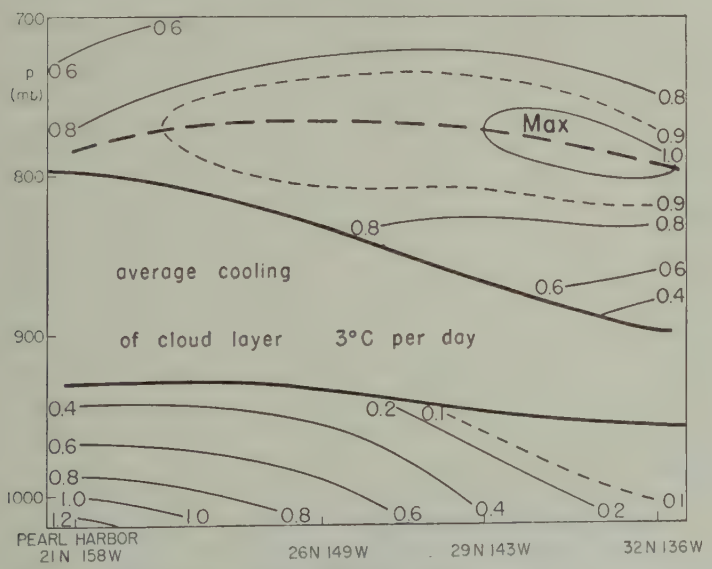


Figure 22. Vertical cross-section of net temperature changes (°C day^{-1}) produced by radiation.

(iv) *Conclusion*

For evaluation of Eq. (7) the following values are now available :

$$R = -1{\cdot}8 \text{ m cal/day}, \quad Q_s = 0{\cdot}3 \text{ m cal/day}, \quad LP = 1{\cdot}8 \text{ m cal/day}.$$

It follows that Eq. (7) gives an almost exact balance. *The export of sensible heat from the region as a whole is small compared to the export of latent heat.* Although each of the preceding calculations is based on approximate procedures, each questionable in some measure, it is difficult to believe that the good agreement obtained for all of them was due to chance.

(d) *Latent heat balance completed*

We shall now complete our picture of the moisture balance in the three layers. For this purpose we assume that the accumulation of sensible heat is zero not only for the region as a whole but also for each layer individually. As Fig. 11 shows, this is not exactly true. Near the ground the potential temperature of the air increases downstream and higher up it decreases slightly. This indicates that there is a net downward flux of sensible heat, since Q_s is just sufficient to balance the radiational-heat loss of the lowest layer and since there is no other heat source for this layer. It follows that the turbulent transfer of moisture upward, to be computed presently, may be somewhat too small in the higher layers.

We now have the following values for the vertical flow of moisture (*cf.* Fig. 23 and Table I) : 4·9 m cal/day from ocean to atmosphere (Q_e) ; 4·1 m cal/day through the top of the lowest layer.

The transfer through the top of cloud and inversion layers depends on the amount of precipitation in each layer or, more strictly, $E - P$. We assign as much rainfall as is required to balance the calculated radiational-heat loss in the middle and upper layers, and then obtain a flux of : 1·2 m cal/day through the top of the middle layer, and 0·5 m cal/day through the top of the section. This completes

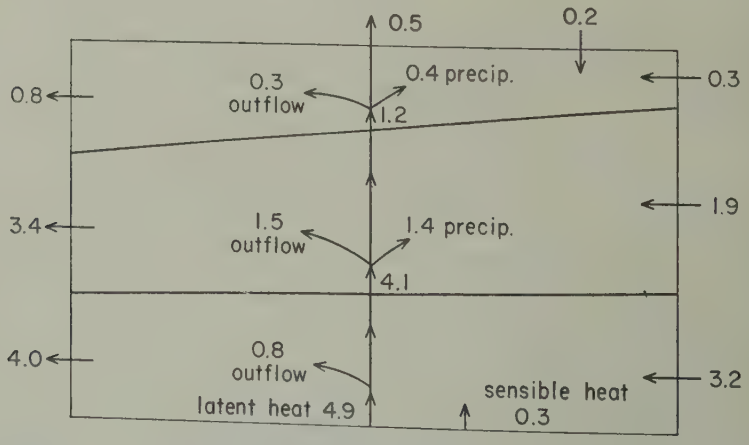


Figure 23. The moisture budget (units 10^{19} cal) taken with respect to the three layers shown in Fig. 9

the picture of Fig. 23. It is seen that about 70 per cent of the rainfall comes from the middle layer, which includes the bulk of the cloud layer. This is a reasonable result, in good agreement with qualitative estimates of the vertical distribution of rainfall. It does not appear profitable to pursue the subject at this time beyond the overall balance given. Many interesting problems remain. One of these is the heat balance over short periods, say one week. The rainfall of our region is by no means steady (Riehl 1949, 1950). Most of it falls on a very small number of days at any given location. Extended dry periods intervene. It is only with the aid of a new and specialized programme of observations that the problem of these shorter periods can be attacked.

As stated earlier, the net latent-heat export from the section is 3·1 m cal/day. If we wish to determine whether there is a net gain of heat available to maintain heat balance in other parts of the world, we must take into account at least the whole troposphere. The radiation calculations for the layer 3 km to 13 km (700 — 200 mb) show a net cooling of 0·7° C/day, or a heat loss of 2·0 m cal/day over the whole section. It follows that a residual actually exists (1·1 m cal/day) which is available for net export. This residual is about 20 per cent of the heat transferred from ocean to atmosphere.

4. Turbulent exchange within the trade

(a) *Dissolution of the trade inversion*

The observations presented in this report have shown that the trade inversion rises one km from inflow to outflow end of our section despite continuous divergence and sinking of the air columns. This necessitates a revision, at least for the region under consideration, of previous hypotheses on the breakdown of the inversion. These hypotheses had postulated that no mixing takes place between the masses below and above the inversion until the inversion is destroyed. Actually however a continuous mass transfer takes place across the inversion (Fig. 9). The outflow of mass between sea level and the base of the inversion at Honolulu is $11{\cdot}2 \times 10^9$ g/day, the inflow at 32° N., 136° W. is $5{\cdot}9 \times 10^9$ g/day, and the total downward transfer through the inversion base is $5{\cdot}3 \times 10^9$ g/day. As the air needs six days to travel the distance, an average of 22 g of each air column of unit cross-section is incorporated daily into the cloud layer.

It has already been suggested by J. Wyman (Harvard University) *that the rise of the inversion is accomplished through the pick-up of latent heat by the trade* in the course of its passage over the tropical ocean. The present data confirm and extend this hypothesis. We may take the position that the inversion is 'lifted' by the process indicated in Fig. 24. It is well known that the bases of the cumuli have a nearly uniform height, but that the tops are very irregular. Some are found within the cloud layer, many near the inversion base, and some within the inversion layer as active clouds penetrate the base. As shown by visual observation and many photographs, the tops of these clouds break off and evaporate quickly. In this way moisture is introduced into the lower portions of the inversion layer, and the air there situated gradually takes on the characteristics of the cloud layer.

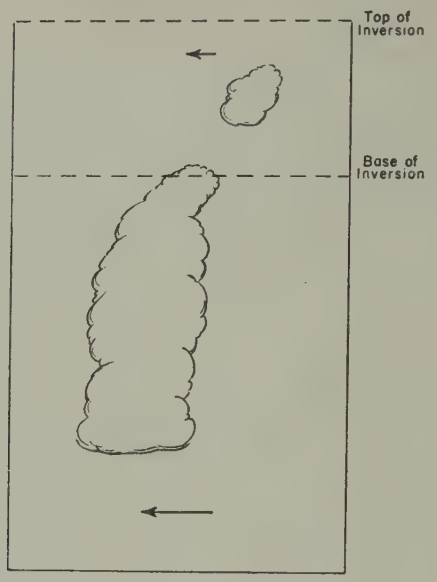


Figure 24. Illustrating the mechanism for raising the trade inversion. For a similar suggestion on the role of cumulus clouds, see Namias (1939).

As shown by Bunker *et al* (1949), eddy diffusion is the main agent for vertical transport of latent heat in the subcloud layer. In the cloud layer, cumulus convection also must be included. We can estimate whether such convection alone is capable of effecting the required upward transport of latent heat, *i.e.*, we can see what order of magnitude of vertical motion is necessary. For this purpose, we choose a reference level located within the cloud layer. The latent energy transfer (F) through this level is a value intermediate between 1·2 and 4·1 m cal/day (Fig. 23). Its exact value cannot be determined. If we assume that the higher value of 4·1 m cal/day still holds approximately in the middle of the cloud layer, we shall obtain vertical motions which are too large. If these motions nevertheless are reasonable, we can be quite certain that cumulus convection is capable of transporting the smaller latent energy amount actually transferred. The equation for transfer of latent heat across the reference level is

$$F = \sum_a w_a \rho_w{}^s \Delta_a - \sum_c w_c \rho_w{}^s \Delta_c - \sum_d w_d \rho_w \Delta_d \quad . \quad . \quad (9)$$

Here the subscript a refers to the actively ascending cloud portions, c to the stationary or descending cloud portions, and d to the descending clear air. Δ_a, Δ_c, Δ_d are the area elements, and w_a, w_c and w_d the corresponding vertical motions. The clouds are considered saturated (vapour density $\rho_w{}^s$), whereas the clear air has the mean vapour density ρ_w. Since this is an order-of-magnitude calculation, the liquid-water content, the vertical component of the mean motion (order of 10^{-3} m/sec), and several details resulting from entrainment considerations are neglected. Introducing the mean density ρ_m of the cloud layer, and replacing the summations by mean values,

$$\frac{F}{\rho_m} = (\bar{w}_a \Delta_a - \bar{w}_c \Delta_c) q_s - w_d \Delta_d q \quad . \quad . \quad . \quad (10)$$

The equation of continuity is

$$\bar{w}_a \Delta_a - \bar{w}_c \Delta_c = \bar{w}_d \Delta_d \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Introducing Eq. (11) into Eq. (10),

$$\bar{w}_d = \frac{F}{\rho (q_s - q) \Delta_d} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

All quantities necessary for evaluation of Eq. (12) are known. $F = 790$ g/sec ; $q = 8{\cdot}9$ g/kg (Fig. 13(a)) ; $q_s = 11{\cdot}5$ g/kg (Fig. 10(a))*; $\rho = 1{\cdot}1 \times 10^{-3}$ g cm^{-3} and Δ_d is one half of the whole area since the mean cloudiness is 50 per cent. It follows that $w_d = 2{\cdot}3$ cm/sec, a very reasonable value (*cf.* also Bunker *et al.* 1949). From Eq. (11),

$$\bar{w}_a = \bar{w}_c \Delta_c + \bar{w}_d \Delta_d / \Delta_a \quad . \quad . \quad . \quad . \quad (13)$$

To evaluate $\bar{w}_a$ it is necessary to make assumptions about $\bar{w}_c$ and Δ_a, noting that $\Delta_a + \Delta_c = \Delta_d$. The inactive cloud matter may stay at one level as is often observed, or it may descend at a rate that is not likely to exceed w_d when averaged over all clouds. We shall set $0 < \bar{w}_c < \bar{w}_d$ and evaluate $\bar{w}_a$ for both extreme cases. Using at first $\Delta_a = 2{\cdot}5$ per cent of the whole area (5 per cent of the area covered by clouds), $\bar{w}_{a\,\max} = 90$ cm sec^{-1} and $\bar{w}_{a\,\min} = 46$ cm sec^{-1}. These values are very low as the rate of ascent of active cloud towers is known to have the order of m sec^{-1}. Using $\Delta_a = 1$ per cent of the whole area for a second estimate (2 per cent of the cloud area), $\bar{w}_{a\,\max} = 2{\cdot}2$ m sec^{-1} and $\bar{w}_{a\,\min} = 1{\cdot}2$ m sec^{-1}. This shows clearly that *cumulus convection is entirely capable of transporting upward at least the major portion of the energy added to the trade air flowing south-westward over the tropical oceans.* Further detailed observations are needed however that will permit more refined calculations, including such factors as evaporation and down draughts.

(b) *The balance of momentum*

In the following we shall calculate the momentum balance in our section. If the equation of relative motion along a streamline

$$\frac{dv}{dt} = -\left(\frac{1}{\rho}\frac{\partial p}{\partial s} - f\right) \quad . \quad . \quad . \quad . \quad . \quad (14)$$

is transformed analogously to Rossby and Starr (1949), we obtain

$$\frac{\partial(\rho v)}{\partial t} + \operatorname{div}(\rho v \mathbf{v}) = -\frac{\partial p}{\partial s} + f \quad . \quad . \quad . \quad . \quad (15)$$

Here the co-ordinate s is taken along the streamline shown in Fig. 1, but is defined positive toward ENE. ; f is the frictional force per unit volume and v the three-dimensional velocity vector. Integrating over a volume bounded by ds, dz, and of unit thickness in the direction normal to s,

$$\frac{\partial}{\partial t}\int\int \rho v \, ds \, dz + \int\int \operatorname{div}(\rho v \mathbf{v}) \, ds \, dz = \int\int \left(-\frac{\partial p}{\partial s} + f\right) ds \, dz \quad . \quad (16)$$

* The mean cloud temperature is considered equal to the mean clear-air temperature (Stommel 1947 ; Austin 1948). Density differences between ascending and descending cloud elements cannot be included.

As the momentum in the volume does not change from beginning to end of the period studied, the local-change term vanishes. The frictional force f is closely given by

$$f = \frac{\partial}{\partial z}\left(\mu \frac{\partial v}{\partial z}\right),$$

where μ is the coefficient of turbulent momentum exchange. Evaluation of the integrals of Eq. (16) then gives

$$\int_u \rho v v\, dz - \int_d \rho v v\, dz + \int_t \rho v w\, ds - \int_b \rho v w\, ds + \int (p_u - p_d)\, dz - \int \left[\left(\mu \frac{\partial v}{\partial z}\right)_t - \left(\mu \frac{\partial v}{\partial z}\right)_b\right] ds = 0 \quad . \quad . \quad . \quad (17)$$

Here, the subscripts u and d indicate upstream and downstream respectively ; t and b refer to top and bottom of the layer.

Evaluation of Eq. (17) is difficult since it contains non-linear terms. At the surface, the frictional term $\left(\mu \frac{\partial v}{\partial z}\right)_0 = k\rho\, v_a^2$ where v_a is the total velocity at ship's deck level and k is taken as 0·003. This term was computed twice daily at each of the four observation stations, and its component along s was found by multiplication with the cosine of the angle between the individual wind direction and s. We also evaluated the horizontal-transport terms from the 12-hourly pilot-balloon observations, but could not do so in case of the vertical-transport terms. Since daily values of the vertical motion were not available, we used the mean motion (Fig. 7) for calculation of the vertical-momentum flux. As Table III will bring out, this approximation is not likely to be important.

For the investigation of the momentum balance and the subsequent comparison between exchange coefficients of moisture and momentum it is of most interest to calculate with respect to the characteristic layers of the trade-wind belt (Fig. 8). We choose the height corresponding to the mean height of the 960 mb level for the subcloud layer ; 880 mb for the cloud layer ; 800 mb for the inversion layer ; 720 mb for the air above the inversion. The procedure consists in calculating all known terms of Eq. (17) for the layers 1,020-960 mb, 960-880 mb, 880-800 mb, and 800-720 mb. In case of the lowest layer, there is then only one unknown term, the turbulent-momentum transfer at the top, and this term therefore can be computed. From there, we can solve Eq. (17) for all layers. Table III summarizes the result.

TABLE III. VALUES OF THE TERMS IN EQ. (17) (10^8 G CM SEC^{-2})

Layer (mb)	Inflow upstr.	Outflow downstr.	Net	Inflow top	Outflow bottom	Net	Pressure term	Turb. bottom	Turb. top
1,020-960	− 0·3	− 0·2	− 0·1	0	0	0	− 2·7	4·4	− 1·6
960-880	− 0·4	− 0·4	0	− 0·1	0	− 0·1	− 2·6	1·6	1·1
880-800	− 0·4	− 0·4	0	− 0·1	− 0·1	0	− 1·2	− 1·1	2·3
800-720	− 0·4	− 0·3	− 0·1	− 0·1	− 0·1	0	0	− 2·3	2·4

We can immediately conclude from this table that the advective terms are of no importance, and that essentially we have a balance involving only pressure and frictional forces. The pressure drop along the trajectory accelerates the air toward WSW., and this acceleration is counteracted by friction. It is of interest that the pressure gradient becomes zero in the top layer, *i.e.* the air flows along the isobars (*cf.* Fig. 6(b)). Previously we had noted that the divergence, prevalent in the lowest layers, also disappears near the top of the section. Thus these calculations give internally consistent results.

From the last column of Table III we can calculate the characteristic coefficients of turbulent-momentum exchange (μ) within each of the four layers. At the top of the lowest layer (960 mb), $\mu \dfrac{\partial v}{\partial z} ds = -1{\cdot}6 \times 10^8$. As $\partial v/\partial z$ is still negative at that level (Fig. 5(a), the negative signs cancel. An excess of momentum toward ENE., picked up from the ground is being transmitted upward through the 960 mb level. After calculation of $\partial v/\partial z$ from Fig. 5(a), we find that $\mu_{960} = 370$ g cm^{-1} sec^{-1}.

At the top of the second layer (880 mb), $\mu \dfrac{\partial v}{\partial z} ds = 1{\cdot}1 \times 10^8$. At this level, $\partial v/\partial z$ has reversed sign. The easterlies now decrease upward. Thus μ remains a positive number and its value, computed as before, is 630 g cm^{-1} sec^{-1}. We can say that the momentum transported through the bottom of the section is insufficient to balance the acceleration due to the pressure force, and that therefore we must import additional momentum from above. *We see that there is a turbulent transfer of appreciable magnitude and that this transfer is directed from high toward low momentum. Where the easterlies increase with height, the transfer is upward. Where the easterlies decrease with height, the transfer is downward.* As stated previously (Riehl and Yeh 1950), *the trades receive westerly momentum both from above and below.*

It follows from the foregoing that the momentum picked up by friction from the ground should be just used up at the level where the ENE. wind is strongest (910 mb). At that level $\partial v/\partial z = 0$. Therefore there is no turbulent-momentum transfer through this level. The balance for the whole layer (units as in Table III) is as follows : gain from skin friction 4·4, loss due to net horizontal inflow 0·1, loss due to pressure-gradient force 4·5, balance $-0{\cdot}2$. We see that the gains equal the losses within the limits of accuracy of the observations.

(c) *Comparison between momentum and water vapour exchange coefficients*

The *Austausch* coefficient, A, for water vapour can be calculated from Eq. (6) in a manner similar to that of the preceding section. Table IV shows the results, including the ratio μ/A.

TABLE IV. *Austausch* COEFFICIENTS OF WATER VAPOUR (A) AND MOMENTUM (μ)

Level (mb)	A (cgs)	μ (cgs)	μ/A
960 (subcloud)	100	370	3·7
880 (cloud)	62	630	10
800 (inversion)	5-15	680	$\simeq$ 70
720 (above-inversion)	0-5	570	$\simeq$ 120

At 800 and 720 mb, the value of A depends on the exact vertical distribution of rainfall which is unknown. Therefore the possible range has been indicated, corresponding to extreme cases of allocation of precipitation. Evidently, this does not affect the outstanding result : *both μ and A vary greatly with height. A decreases upward and μ increases upward, at least to the inversion layer. Thus the ratio μ/A becomes large above the cloud layer.* The values of μ are greater than those of Sverdrup (1917) who computed $\mu = 140$ for the layer 0-1,200 m (below the inversion), 260 between 1,200 and 1,800 m (inversion layer) and 90 above the inversion. In case of A, we can compare our values with those of Bunker *et al.* (1949) who obtained 200-400 g cm^{-1} sec^{-1} in the subcloud layer and 100 g cm^{-1} sec^{-1} in the cloud layer. In this instance, our values are the lower ones. In both cases however the order of magnitude is the same and the coefficients vary in a similar way with height. A decreases upward from the subcloud layer as computed both by Bunker *et al.* and ourselves. On the other hand, μ at first increases upward to the inversion where it reaches a maximum, and then it begins to decrease. This is shown by Sverdrup's study and by our calculations.

The interpretation offered for the ratio μ/A as computed rests on the fact that several mechanisms can provide for vertical momentum exchange, but that only one of these transports water vapour. Jacobsen (1930) already had noted in a study of vertical mixing of salt and momentum in water that the exchange coefficient of momentum was five times larger than that of salt. For the atmospheric case, we may consider a bubble of moist air with low horizontal momentum that ascends through a drier environment with higher momentum. The horizontal momentum of the rising mass can be imparted to the surroundings by form-drag forces such as experienced by a pilot balloon. It can also be imparted by fragmentation at the edges and gradual mixing of the mass itself with the environment. Clearly, only the second of these processes exchanges moisture. Therefore small eddies are most efficient for moisture transport. A rising column of large diameter can exchange momentum with its surroundings by means of form-drag and pressure forces. Moisture, however, is mixed only by small sub-eddies at the periphery and the column can sink back to a level near its origin, retaining most of its moisture.

There are definite indications that the dimension of the eddies increases as we go upward from the surface. Very near the ground, the exchange coefficients are known to be nearly identical. In the subcloud layer (Bunker *et al.* 1949) eddies of 50-150 m diameter predominate. In the cloud layer, the diameter has the order of kilometers (*cf.* Langwell 1948). The rapid decrease of wind steadiness above the inversion base (Fig. 3) suggests that the eddy size continues to increase with height until we reach the upper troposphere, which is characterized by wave or vortex trains with dimensions of the order of 1,000 km.

In the subcloud layer, the stratification of the air is so slight that eddy diffusion of moisture by the small turbulent elements observed is very effective. Near the base of the cloud layer, these small eddies die out rapidly as the stability of the air increases (Bunker *et al.* 1949 ; Langwell 1948). Most of the mixing in the cloud layer takes place through convective cells, and it is the intensity of this process of mixing that A measures in this layer. Thus A does not represent a discrete physical mechanism, but it is mainly a convenient tool of calculation.

The rapid decrease of the coefficient in the inversion layer is readily explained by the large stability that damps out all but very strong eddies. This stability acts as a shield for the upper air against penetration of effects of surface roughness and most of the turbulence caused by convective activity in the cloud layer. Thus A continues to become smaller even above the inversion layer. The smooth travel of airplanes above the inversion top is well known. In the middle and upper troposphere, vertical moisture transfer probably takes place only in synoptic disturbances.

Regarding the coefficient of eddy viscosity, μ, it is possible merely to make a suggestion concerning the variation with height. The form drag on an air parcel depends directly upon its mass. It seems logical to try a relation of the form $\mu \propto r^2 w$, where r is the radius and w the vertical velocity of a rising eddy. Now r is known to become greater with height, whereas w decreases. As long as the increase of r^2 is more than the decrease of w, the coefficient of momentum transfer may be expected to increase with height. Our data point to the possibility that $r^2 w$ reaches a maximum value in the inversion layer.

5. Conclusion

This report has shown that the trade-wind inversion rises downstream in a large-scale field of horizontal divergence and sinking of the air columns. Thus a revision of previous statements on the dissolution of the inversion is required. Convection furnishes an adequate mechanism for this purpose. Throughout each day a very large number of cumuli that carry moisture upward reach the inversion base and push a smaller or greater distance into the inversion layer. Here the cloud towers mix with the dry surroundings. Bit by bit, the lower part of the inversion layer assumes the characteristics of the cloud layer and is absorbed into it.

The upward flux of latent heat requisite for the turbulent mixing effected by the upper parts of the trade cumuli is obtained through a calculation of the balance of of latent and sensible heat. A balance is obtained that yields reasonable values for A, the coefficient of turbulent-moisture exchange, as a function of height. This balance also shows, in agreement with earlier views on the trade wind region, that a net export of latent heat takes place from the area. This heat is available to balance radiational-heat losses elsewhere. It amounts to about 20 per cent of the heat transferred from ocean to atmosphere.

Finally, the momentum balance is calculated, and the intensity of vertical mixing of momentum is compared with that of moisture. Near the ground the turbulent flux of momentum is upward, as was to be expected. Higher up however it is downward and the intensity of downward mixing increases with increasing height. Thus this mixing turns out to be a principal brake on the development of high easterly and westerly velocities in the meridional cell of the trade region. This result again shows that it will not be possible to obtain a solution for the general circulation, at least in low latitudes, by considering two-dimensional horizontal-flow models alone.

In the course of the calculations, it was necessary to employ several approximate procedures, especially for the computation of evaporation and radiation. It is certainly of interest that these different approximations and assumptions add up to a reasonable balance. But it is also clear that the calculations here performed should

be repeated with the aid of a special programme of observations. Such a programme will make possible to eliminate a large portion of the assumptions made, and it should yield a final picture of the balance of sensible heat, moisture and momentum in atmosphere and ocean of the trade-wind region.

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Point-discharge currents and the earth's electric field

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Summary

An account is given of measurements of the current flowing to earth through a point set up at a height of 12 m, together with the electric field near the ground. Point-discharge current of either sign may be represented approximately as increasing with the square of the field, as found by Whipple and Scrase. In the light of recent work on corona discharges between a point and a plane in air at atmospheric pressure it is suggested that for high fields the point-discharge current will vary directly as the field. Irregularities in the curves relating averages of discharge current and field are attributed to space charge arising from local point discharge at or near the raised apparatus used for measuring the field. An advancement of field changes ahead of the corresponding point-current changes is considered to be an example of the reversal, by space charges, of the sign of field between the level of a raised point and the ground, described by Lutz.

1. Introduction

In the course of a study of rainfall electricity at Durham (Hutchinson and Chalmers 1951) a number of measurements of the discharge current through an insulated raised point were made, together with the value of the vertical electric field near the ground. Certain interesting features may be compared with the findings of three other investigators.

Whipple and Scrase (1936), at Kew, recorded the discharge current through an insulated point set up 8·4 m high on a pole, and simultaneously the vertical field was measured with a polonium collector rod 2·5 m high jutting out horizontally from the wall of a hut 6 m high. From the records for six thunderstorms they selected about 200 periods in which the discharge current was fairly steady, and plotted the mean values of current and field for each period on a graph. They found an approximate empirical relationship $i = a(F^2 - M^2)$ where i is the downward directed current in μa, F the downward directed field in v/cm, M the minimum field for the onset of point discharge, and a a constant depending on the sign of current.

Chiplonkar (1940), working at Colaba, Bombay, measured the current through a point 8·3 m high, and recorded the field simultaneously using a radioactive collector, during the monsoon season 1938. Most of his observed currents were less than ·3 μa. He represented his results graphically with a curve nearly identical with that given by Whipple and Scrase's relationship. He obtained some evidence that the total current through a bunch of four points was less than that through a single point at the same height and in the same field.

Lutz (1941) recorded the discharge current through a point 17·2 m above ground. In addition to the field at the ground he also measured the space charge. There were periods as long as half an hour during which point-discharge current and field were of opposite sign. The space-charge record at such times showed that

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ions of the same sign as those being discharged from the point were present in sufficient density to change the sign of the field near the ground from that at the level of the discharging point. Thus in one prolonged period of positive point-discharge current the field at 17 m must have been positive, yet near the ground it was negative, in keeping with the recorded negative space charge which the positive point-discharge current would produce. Lutz concluded that the field at the ground alone is an unreliable guide to the charge distribution in a storm cloud. One should also measure the point-discharge current at various heights and the space charge. His paper includes an extensive bibliography.

2. Experimental details

For the measurements reported here the point consisted of 1 cm of fine platinum wire at the upper end of a stout copper wire 30 cm long. This was insulated from and fastened to the top of a 1·5 m wooden post on the corner of a building some 20 m by 40 m by 10 m high, the point being 12 m above ground. Parts of the building stood 2 or 3 m higher than the point, and there were lower trees and buildings round about. An insulated cable carried the current through a moving-coil mirror galvanometer to earth, a shunt giving a choice of two sensitivities. The deflexions were recorded photographically. From time to time the platinum wire forming the point would be bent over, presumably by birds. (Dr. Leckie of Bandoeng states that he has seen birds dive at and bend a similar point). The point was observed daily and straightened when necessary. Later it was replaced by a 2 mm-diameter brass rod tipped with platinum and brought to a sharp point. This remained quite firm, and the few observations made with it showed no obvious divergence from those made previously.

The field was measured by a bound-charge method. The apparatus was mounted 17 m horizontally from the discharging point and at a height of 2·5 m on the flat roof of a hut 3 m square and connected by a cable to a galvanometer inside. On exposing or screening a metal surface connected to a valve amplifier, a ballistic throw proportional to the bound charge, and hence to the field, was observed visually or recorded on the same drum as the point current. Observations when performed visually were made about once in five seconds or, when recorded, about twice in twenty seconds, unevenly spaced, this rate being suitable for the observations on rain. Calibrations were made in an open space with a stretched wire 1 m high and a glowing fuse collector.

3. Results

Nearly 1,000 simultaneous observations of point-discharge current and field were made on 14 days with rain in 1948 (3 with thunder) and during one snowfall on 7 March 1949. They showed a distribution, with much scatter, resembling that obtained by Whipple and Scrase (*loc. cit.*). The range of field values was next divided into convenient intervals – 5 v/cm except for the highest fields – and the values of current observed within each interval were averaged to give one point on a graph, Fig. 1. The number of observations represented by each point is shown.

There are two prominent ' humps ' between + 10 and + 30 v/cm and between − 10 and − 50 v/cm. The maximum current observed was + 20 μa during the snowfall of 7 March 1949.

On many occasions a change in the field at the ground was ahead of the corresponding change in point current. At first sight, when observing visually, it looked as if the point-discharge galvanometer coil had stuck. This effect was pronounced on four occasions when the field changed sign 15 to 20 sec before the point current fell to zero. The details are given in Table I.

TABLE I

Date (1948)	Field change	i, at $F = 0$, μa	Approx. interval, $F = 0$ to $i = 0$, sec	F, at $i = 0$, v/cm	F, when i begins with changed sign, v/cm
4 Apr.	+ to −	+ 0·7	20	− 15	—
4 Apr.	− to +	− 0·1	15	+ 20	+ 20
3 May	+ to −	+ 0·2	20	− 15	− 30
14 June	+ to −	+ 1·4	15	*c.* − 5	—

4. Discussion

(i) *Approximate agreement with the Whipple and Scrase square law*

Although all the measurements were used to obtain Fig. 1, without regard to whether the point current was steady or not, the curves have much in common

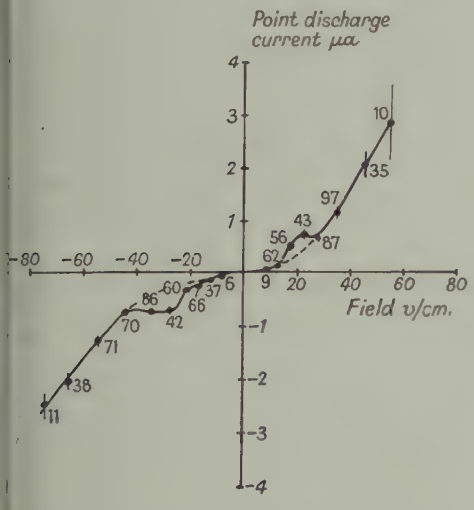

Figure 1. Average values of point-discharge current at Durham. The number of observations is shown against each point. The short vertical lines represent twice the standard error for each point.

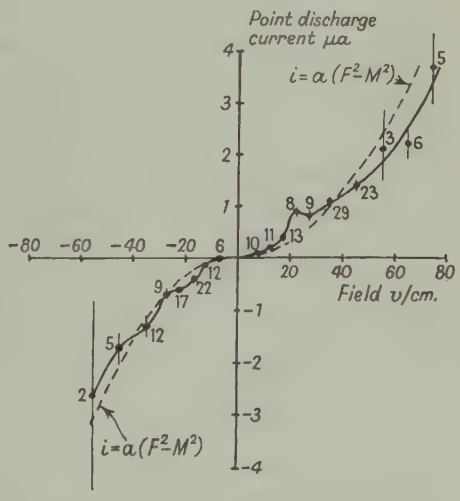

Figure 2. Average values of point-discharge current obtained from Kew results. The number of observations is shown against each point. The dashed curve represents the empirical relationship $i = a(F^2 - M^2)$. The short vertical lines represent twice the standard error for each point.

with those drawn by Whipple and Scrase. These authors found $M = 7{\cdot}8$, $a = 0{\cdot}0008$ for positive currents, and $M = 8{\cdot}6$, $a = 0{\cdot}0010$ for negative currents. The curves of Fig. 1 (ignoring the humps for the moment) yield $M = 4$ for either sign, and $a = 0{\cdot}0009$ for positive currents and $a = 0{\cdot}0005$ for negative currents. It is reasonable that the Durham measurements give a lower value of M since the point was 3·5 m higher than that at Kew. The factor a undoubtedly depends on the surroundings of the point, though the values at Durham and Kew are of the same order of magnitude. From Fig. 1 it appears that for 2 μa the negative field is 50 per cent higher than the positive field. On the other hand, at Kew the positive field *exceeds* the negative value for a given current (Fig. 2).

Although there appears to be no theoretical account of the apparently parabolic form of the point-discharge current/potential gradient curve as indicated in Figs. 1 and 2, there is evidence that the square law applies to a limited range only of field values. Recent laboratory experiments on point-to-plane discharges in air at atmospheric pressure, reviewed by Loeb (1948), show that for a gap of 3 cm and a potential difference of 5,050 v a corona discharge is set up at some 0·3 or 0·4 μa. The current consists of a series of pulses, each one extinguished by the space charge it creates, and the current is found to vary directly with the applied potential difference. This mechanism suggests that for high fields the point-discharge curve will show a *linear* relationship with field, and this is in keeping with the average values for the highest fields, of either sign, shown in Fig. 1.

(ii) *Effects of space charge below the point.*

The humps in Fig. 1 may be regarded as displacing the curves beyond them away from the current axis, by about 15 v/cm for positive fields and 25 v/cm for negative fields. Some such displacement would be expected if in conditions corresponding to the humps there were a space charge lying between the discharging point and the field-measuring apparatus and of such sign as to tend to reduce the field. This space charge would result from point discharge at the field-measuring apparatus itself as well as as the corners of the hut on which it was mounted, and of course from surrounding shrubs, and would be of the right sign to reduce the field locally. Once this *local* discharge had begun from all the corners and projections of the 3 m square hut, the field as measured would suffer no more rather abrupt depressions and the curves of Fig. 1 would resume their parabolic form, though displaced.

The magnitude of the space charge which would cause the humps may be calculated approximately. If the total space charge per unit horizontal area between the level of the point (12 m) and that of the field-measuring apparatus (2·5 m) is σ esu/cm^2, the resulting change in the field is ΔF v/cm, where

$$\frac{\Delta F}{300} = +4\pi\sigma$$

The positive sign indicates that a positive space charge (as produced in a negative field) increases the measured field algebraically. The *average space charge* ρ in esu/m^3 over the height interval 9·5 m is, therefore,

$$\rho = + \frac{10^4}{9{\cdot}5} \frac{\Delta F}{4\pi \times 300}$$

$$= + 0{\cdot}28\, \Delta F$$

From Fig. 1 the curve displacement at + 0·5 μa is about 6 v/cm towards the current axis, so that $\Delta F = -6$ v/cm and $\rho = -1{\cdot}7$ esu/m^3. For negative currents, at − 0·5 μa, the curve displacement gives $\Delta F = +13$ v/cm and $\rho = +3{\cdot}6$ esu/m^3. These values are of the same order as those measured by Lutz (1941).

Fig. 2 shows curves drawn through points obtained from the results of Whipple and Scrase (*loc. cit.*) by taking average values as for Fig. 1. Humps appear in both positive and negative quadrants. It is to be noted that Whipple and Scrase measured the field by a polonium collector projecting from the wall of a hut. In both Figs. 1 and 2 the negative-quadrant hump extends over a much wider field range than the positive. Perhaps this is due to the lower mobility of positive ions compared with negative, but no definite conclusion can be formed on results so far available.

The observed advancement of a changing field is a less conspicuous example of the phenomenon of contrary point-discharge current and field at the ground observed by Lutz. At Durham, when the field at the discharging-point level began to fall, the field at the ground would also fall, but on account of the space charge due to point-discharge ions this field would actually change sign in advance of the point current, if indeed the latter changed sign at all. As in the case of the humps considered above, the average volume distribution of space charge between the levels of the discharging point and the field-measuring apparatus is

$$\rho = + 0{\cdot}28\, \Delta F$$

From the data in Table I and the curves of Fig. 1 it is possible to calculate approximately the change ΔF in the field for three particular instants :

(a) when the measured field changes sign, (b) when the current becomes zero and (c) when the current begins to flow with reversed sign.

The values of ρ for these times are shown in Table II.

TABLE II

Date	Field Change	(a)	(b)	(c)
4 Apr.	+ to −	− 7·8	− 3·1	—
4 Apr.	− to +	+ 3·1	+ 4·5	+ 4·5
3 May	+ to −	− 3·9	− 3·1	− 7·2
4 June	+ to −	− 10·6	− 0·3	—

As in the case of the humps, the space charges calculated are of the same order of magnitude as those measured by Lutz (1941), though they are on the whole greater than those for the humps, suggesting a greater space charge lying between the 12 m point and the field-measuring apparatus. Now we have associated the humps with the onset of point discharge at the field-measuring apparatus and hut roof, these forming a somewhat isolated projection above the ground, so that a

localized space charge is produced. This same space charge, added to that due to all other neighbouring discharges, up to and including the 12 m point, we consider to result in field advancement. Since the space charges are of one sign, either all positive or all negative, the field-advancement effect should, therefore, yield larger calculated values of space charge than the hump effect, and this is what we found above. Lutz moreover observes that if the space charge is as much as 3 esu/m^3 the opposing field exceeds the original. The space charges in Table II, with one exception, are greater than this value. Lutz's discharging point was probably better isolated and certainly higher (17·2 m) than that at Kew (8·4 m) or Durham (12 m) and consequently his work shows the effect of space charge most clearly.

5. Conclusion

The measurements show that point-discharge current of either sign may be represented roughly as increasing with the square of the field near the ground, as found by Whipple and Scrase. From recent work on corona discharge it appears that for high values of field the point current will increase linearly with field. Space charges sufficient to modify the field as measured near the ground may result from point discharge at the field-measuring device itself and its mountings, unless the apparatus is flush with the ground. Space charges also sometimes reverse the field between the point and the ground, an effect apparently more in evidence with greater height (and exposure) of the discharging point. The Durham results support Lutz's conclusion that the point current at various heights (and space charge) should be measured as well as the field at the ground in order to yield reliable information about storm-cloud electrification.

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Synoptic properties of frontal surfaces

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Summary

The mathematical consequences of a first-order temperature discontinuity at a front are developed for the fields of pressure, temperature, and geostrophic wind. These results are applied to determine the correct construction of synoptic charts in the vicinity of fronts, and to determine criteria for obtaining the boundary levels of frontal surfaces, as well as their orientations and slopes. Techniques for qualitative and quantitative evaluation of vertical motions at fronts are also presented. These concepts, and analytical methods based on them, are illustrated by reference to the synoptic situation over North America on 4 April 1950. Particular attention is paid to the structure of occluded frontal waves, their three-dimensional analysis and representation.

1. Introduction

It should surely be the aim of major or central forecast offices to assimilate and integrate all available data into the best possible four-dimensional (space and time) atmospheric analysis of such elements as temperature, contour height, humidity, and the horizontal and vertical components of air motion.

The great advantage of the polar-front theory lies not in its theoretical basis (still considered inadequate to explain cyclonic development fully) but in the fact that it provides an elastic model of structure, motion, and development by means of which atmospheric conditions can be rationally integrated. At first this model could be applied only to surface representation ; at present it is possible to use the frontal concept to represent a vast amount of significant data in three dimensions. This can be effected two-dimensionally on a frontal contour chart in the same way that a thickness chart represents a mean thermal picture over a fixed pressure layer. Thickness charts can be used directly to study advection over a layer and indirectly to determine development at the base of the layer (Sutcliffe and Forsdyke 1950). Frontal contour charts can be used indirectly to study advection and directly to determine frontal pattern development and associated cyclonic development. Thickness charts serve directly as an integrating factor in forecasting surface and upper-level charts but give little information concerning either general changes at intermediate levels or the actual distribution (and effects) of vertical motions in significant atmospheric sectors in the layer. Frontal contour charts, on the other hand, are only a subsidiary tool in the direct preparation of prognostic charts (Godson 1950d) but do indicate the regions of significant advection at any level in the troposphere and in addition serve for the quantitative evaluation of vertical motions at the upper and lower boundary surfaces of all frontal layers.

In order to perform a frontal analysis in a unique manner, it is necessary to know the mathematical and physical properties of frontal surfaces. Furthermore, these properties serve for the more accurate construction of synoptic charts at the

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surface and aloft. This accuracy is becoming more and more necessary as a result of the increasing number of dynamic computations based on upper-level charts. One of these, dynamic-instability evaluation, may be singled out for special mention.

In recent years it has been increasingly demonstrated that the dynamic stability or instability of the atmosphere is in many ways as significant a phenomenon as static stability or instability. The release of static instability through quasi-vertical motions is important in local weather considerations. The release of dynamic instability through quasi-horizontal motions appears to be important in determining synoptic developments on a much larger scale (Godson 1950a). Recent studies (Godson 1950b) have indicated that marked dynamic instability, if present, will be found in the upper troposphere, and lower stratosphere, immediately to the south of the axis of the 'jet stream' – the narrow belt of very strong westerlies found above the main polar-front surface. This dynamic instability is associated with the strongly baroclinic conditions found in and just above the frontal surface. Palmén (1948) has shown that the vertical axis of the jet stream intersects the frontal surface in the vicinity of the 500 mb level. This indicates very clearly the extreme importance of the accurate construction of 500 mb charts in such areas if computations of the dynamic stability or instability are to be made using contour lines and isotherms. This cannot be done in a purely *a priori* manner owing to the discrete network of observations and to the experimental errors inherent in the observations themselves. As a result, the isopleths of pressure height and of temperature must be constructed to fit qualitatively the known properties of fronts. For this reason it is important that the positions of fronts be shown accurately on upper-level charts from which any type of dynamical deductions is to be made, and that the choice of such positions be in agreement with the properties of frontal surfaces. As an aid in obtaining and maintaining three-dimensional continuity in such a frontal analysis the frontal-contour chart, as described by Crocker, Godson and Penner (1947), has been found to be a valuable analytical tool.

For computations of dynamic-instability parameters it is necessary to know accurately the horizontal gradients of geostrophic-wind components as well as of temperature. To achieve this accuracy the technique of differential analysis is virtually a necessity. The construction of thickness lines is greatly facilitated if frontal contours are available since thickness lines for the layer between p_1 and p_2 will tend to be concentrated between the frontal contours for p_1 and p_2, and to have smaller gradients elsewhere. In addition, the thickness lines will be roughly parallel to frontal contours. In other words, the three-dimensional frontal model implies a general structure of thickness lines with which existing data can be co-ordinated to give the most accurate representation of the atmosphere's thermal structure.

2. General nature of frontal surfaces

The true nature of frontal surfaces was realized as soon as sufficient upper-air data to permit their study became available. Bjerknes (1932) pointed out that, whereas in theory a front should show a discontinuity of zero order with respect to temperature (*i.e.*, an absolute discontinuity of temperature), in practice the discontinuity is of the first order (*i.e.*, an absolute discontinuity in the horizontal or

the vertical temperature gradients at the front and rear of the frontal zone of transition). He considered, however, that on a synoptic scale a frontal surface could be treated as a zero-order discontinuity, owing to the relatively small lateral extent of the zone of transition, and that the classical equation of Helmholtz (1888) should be applicable to determine the slope of the front and to verify the occurrence of a pressure trough at the front.

In connection with this recognized zone of transition, which is more or less sharply delineated at its upper and lower boundaries, Bjerknes and Palmén (1933) identified the upper surface as the true frontal surface since its intersection with the earth gave the position of the surface front. This consistent definition is entirely logical but has not been universally adopted since frequently the lower surface proves easier to identify. The choice of the lower boundary surface is generally impossible, however, in the case of fronts between polar maritime and polar continental air masses in winter since the lapse rate in the continental air is frequently virtually isothermal from the ground to the level of transition to air with a maritime lapse rate. Furthermore, the upper boundary surface is the one of greatest kinematic importance, if it is admitted that the cloud forms of most significance are found in the warm air proper. Thus it is the upper, or true, frontal surface along which the warm air, if sufficiently moist, will ascend to give typical frontal weather. Preliminary studies of dynamic stability and instability in the vicinity of fronts (Godson 1950b) indicate that a very significant discontinuity in dynamic-stability properties exists between the warm air and the air in the frontal transition zone.

Whereas it is true that a zone of 100 mi or so in width is relatively insignificant with respect to the areas usually charted synoptically it is also true that a depth of 100 mb will be significant with respect to the tropospheric layer, some 700-800 mb deep. Moreover, when conditions are being studied in the immediate vicinity of the front, the frontal zone or layer can certainly not be condensed into a single surface across which the temperature changes discontinuously. It would be most desirable to represent the intersection of both boundary surfaces with any chart level under consideration, as has been done, for example, by Nyberg and Palmén (1942). Failing this, the line drawn at any level should represent the transition between the warm air and the frontal zone itself.

It has been shown elsewhere (Godson 1950b) that the frontal zone is very stable with regard to all processes of eddy diffusion (*i.e.*, not literally a mixing zone). Furthermore, the frontal zone is not necessarily a zone of physical transition of air masses. For these reasons it is considered that the most descriptive term to apply to the frontal layer separating two air masses would be one emphasizing the strong baroclinity in that zone. On etymological grounds the term ' hyperbaroclinic zone ' appears appropriate. The term ' frontal zone ' should be reserved for the case of a broad baroclinic zone without sharp boundaries.

3. Fields of pressure and of temperature in the vicinity of fronts

(*a*) *Temperature field and frontal slope*

Previous investigators of frontal surfaces have generally regarded them as surfaces of zero-order density discontinuity when considering their slopes and the

fields of pressure, temperature and three-dimensional airflow but as zones with at best second-order density discontinuities when considering the effects of frontogenesis and frontolysis (Gião 1929, Petterssen 1933).

It will here be assumed that a frontal surface can be adequately represented by two quasi-parallel surfaces of discontinuity of normal temperature gradient, bounding the hyperbaroclinic zone. Horizontal and vertical sections through such a model are represented by the diagrams, (a) and (b) respectively, of Fig. 1. The x-axis is chosen normal to the front, directed from the warm air (W) through the hyperbaroclinic zone (H) to the cold air (C). The conventional frontal surface is shown as a full line where it intersects the two sections, the auxiliary boundary surface as a dashed line. The front is represented as having a slope given by tan θ.

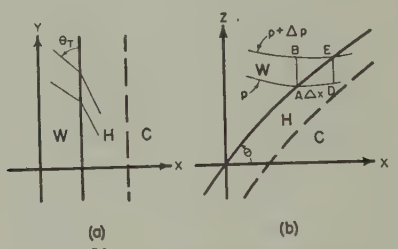

Figure 1. Frontal model ; (a) horizontal section, (b) vertical section.

Consider now a variable ψ which is continuous on passing from region W to region H, or from region H to region C. Consider Fig. 1(a) to represent a sector of an isobaric surface. Let $(\partial\psi/\partial x)_p$ and $\partial\psi/\partial p$ be discontinuous at the two boundaries of the hyperbaroclinic zone ; clearly $(\partial\psi/\partial y)_p$ will be continuous. Consider two isobaric surfaces, specified by p and $p + \Delta p$, which intersect the true frontal surface in Fig. 1(b) and A and E, respectively.

Now
$$\psi_E = \psi_A + \left(\frac{\partial\psi_W}{\partial p}\right)\Delta p + \left(\frac{\partial\psi_W}{\partial x}\right)_p \Delta x$$

Similarly
$$\psi_E = \psi_A + \left(\frac{\partial\psi_H}{\partial p}\right)\Delta p + \left(\frac{\partial\psi_H}{\partial x}\right)_p \Delta x$$

Hence
$$-\left(\frac{\partial\psi_W}{\partial p} - \frac{\partial\psi_H}{\partial p}\right)\Delta p = \left(\frac{\partial\psi_W}{\partial x} - \frac{\partial\psi_H}{\partial x}\right)_p \Delta x \quad . \quad . \quad (1)$$

The isobaric slope of a front, considered positive for both warm and cold fronts, may be defined as, $m_p = -\Delta p/\Delta x$ . . . . . (2)

Thus,
$$m_p = \frac{(\partial\psi_W/\partial x - \partial\psi_H/\partial x)_p}{\partial\psi_W/\partial p - \partial\psi_H/\partial p} \quad . \quad . \quad . \quad . \quad (3)$$

We may now choose $\psi = T$ (temperature). Setting $(\partial T/\partial x)_p = -\beta'$ and $\partial T/\partial p = \gamma'$, so that all quantities will normally be positive except γ' for an inversion,

$$m_p = \frac{\beta_H' - \beta_W'}{\gamma_W' - \gamma_H'} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

At the rear of the hyperbaroclinic zone a similar computation may be performed, and if the slopes are assumed virtually identical, there is obtained

$$m_p = \frac{\beta_H' - \beta_C'}{\gamma_C' - \gamma_H'} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

If the horizontal section in Fig. 1(a) is assumed to represent a constant-level surface, a similar result may be obtained. Setting $\partial T/\partial z = -\gamma$, $\partial T/\partial x = -\beta$ and $\tan\theta = \Delta z/\Delta x$, the corresponding relations are

$$\tan\theta = \frac{\beta_H - \beta_W}{\gamma_W - \gamma_H} = \frac{\beta_H - \beta_C}{\gamma_C - \gamma_H} \quad . \quad . \quad . \quad (6)$$

The relation derived for the slope of the rear or auxiliary bounding surface may also be applied to obtain the slope of the tropopause which represents a surface separating tropospheric air with normal lapse rates (*i.e.*, subscript C) from stratospheric air above with very small lapse rates (*i.e.*, subscript H).

As an example of the application of these equations we may compute the vertical-temperature-gradient discontinuity, corresponding to the mean horizontal-temperature-gradient discontinuity at 500 mb, as evaluated from an earlier study (Godson 1950b, Fig. 3). Assume the frontal zone to be isothermal ($\gamma' = \gamma = 0$) and take $\Delta\beta \simeq \Delta\beta' = 6{\cdot}5°\text{C}/100$ mi as the horizontal-temperature-gradient discontinuity. Assume a value of 10^{-2} for $\tan\theta$ and of 1 mb/mi for m_p.

Thus from Eq. (4), $\gamma_W' = 6{\cdot}5°\text{C}/100$ mb

Similarly from Eq. (6), $\gamma_W = 4{\cdot}2°\text{C/km}$.

These lapse-rate discontinuities are relatively small. The mean slope for the data used was not calculated but probably was considerably smaller than 10^{-2}, giving correspondingly greater lapse-rate discontinuities. The data do indicate, however, that the lapse rate immediately above the frontal surface is frequently more stable than at higher levels or at the same level, away from the front. This is generally true if the warm air is ascending at the front but is less marked if the warm air is subsiding.

In Eqs. (4), (5), and (6) the numerators and denominators will all be positive, corresponding to weaker horizontal but stronger vertical temperature gradients in the warm (or cold) air than in the hyperbaroclinic zone. If a front is to appear sharp in the vertical, the lapse-rate discontinuity ($\Delta\gamma$ or $\Delta\gamma'$, say) must have a relatively large value ; if it is to be clear cut in the horizontal, the horizontal-temperature-gradient discontinuity ($\Delta\beta$ or $\Delta\beta'$, say) must be relatively large. It follows, then, that for a given $\Delta\gamma$ a front will be sharper in the horizontal if its slope is steeper ; for a given $\Delta\beta$ the front will be sharper in the vertical if its slope is more shallow. Hence a cold front (steep slope) will be better defined in the horizontal than a warm front (shallow slope), if the vertical discontinuities are comparable. Similarly a warm front will be better defined in the vertical than a cold front, if the horizontal discontinuities are comparable. Although the vertical and horizontal discontinuities cannot simultaneously be comparable, experience shows that both of the above conclusions are in general true. It will be noted that a front which is only weakly

revealed by a tephigram analysis, which assesses $\Delta\gamma$, may well be clearly defined by a hodograph analysis, which assesses $\Delta\beta$ through the thermal wind relations ; and vice versa.

The above statements may be partly verified by a consideration of the strongly and weakly baroclinic frontal situations at 500 mb of the previous study (Godson 1950b). Assume that the strongly baroclinic cases represent cold fronts with a mean slope of 2 mb/mi ($\tan\theta \simeq 2 \times 10^{-2}$) and that the weakly baroclinic cases represent warm fronts with a mean slope of 0·5 mb/mi ($\tan\theta \simeq 0{\cdot}5 \times 10^{-2}$). For these cases the appropriate values of $\Delta\beta'$ were : cold fronts, $\beta_H' - \beta_W' = 8{\cdot}4°\text{C}/100$ mi ; warm fronts, $\beta_H' - \beta_W' = 3{\cdot}2°\text{C}/100$ mi. The application of Eq. (4) yields the following results for the corresponding lapse-rate discontinuities : cold fronts, $\gamma_W' - \gamma_H' = 4{\cdot}2°\text{C}/100$ mb ; warm fronts, $\gamma_W' - \gamma_H' = 6{\cdot}4°\text{C}/100$ mb. These results are undoubtedly of the correct order of magnitude and corroborate the contention that warm frontal surfaces are frequently sharper in the vertical than cold frontal surfaces, as far as the field of temperature is concerned.

(*b*) *Pressure field at a front*

The consequences of a first-order temperature discontinuity may now be studied for the pressure distribution, *i.e.*, for the field of isopleths of height contours on a constant pressure chart, symbolized by z_p.

Now,
$$\frac{\partial z_p}{\partial p} = \left(\frac{\partial p}{\partial z}\right)^{-1} = -(g\rho)^{-1}$$

where ρ represents air density and g the acceleration due to gravity. Since on a constant-pressure chart the density is continuous at the frontal boundaries, so is $\partial z_p/\partial p$ and also $\partial z_p/\partial x$, as can be seen by setting $\psi = z_p$ in Eq. (3). Also $\partial z_p/\partial y$ is continuous. The isobaric geostrophic-wind equations are :

$$\frac{\partial z_p}{\partial x} = \frac{l}{g} v_g, \quad \frac{\partial z_p}{\partial y} = -\frac{l}{g} u_g \qquad . \quad . \quad . \quad . \quad (7)$$

where l represents the Coriolis parameter, and u_g and v_g represent the x and y components of the geostrophic wind, in this case respectively normal and tangential to the front. Since these two components are continuous, both the direction and speed of the geostrophic wind are also continuous. This implies that no sharp V trough can exist at a front with respect to height contours or to isobars. In other words, the isobars at a front must have a common tangent, analogous to a conventional frontless trough where at any point the geostrophic-wind direction changes continuously.

Although isobars or pressure-height contours show no change in direction on crossing the two frontal boundaries they do exhibit a change in curvature as will now be demonstrated. Let κ_p denote the curvature of the height contours, θ_g the direction of the geostrophic wind (measured counterclockwise from the x direction) and V_g its speed, and τ an axis tangential to the geostrophic wind. By definition,

$$\kappa_p = \frac{\partial\theta_g}{\partial\tau} = \frac{u_g}{V_g}\frac{\partial\theta_g}{\partial x} + \frac{v_g}{V_g}\frac{\partial\theta_g}{\partial y}$$

Since $\partial\theta_g/\partial y$ is continuous across the front, it follows that

$$\kappa_{pH} - \kappa_{pW} = \frac{u_g}{V_g}\left(\frac{\partial\theta_{gH}}{\partial x} - \frac{\partial\theta_{gW}}{\partial x}\right)$$

Using Eq. (3) by setting $\psi = \theta g$,

$$\kappa_{pH} - \kappa_{pW} = \frac{m_p u_g}{V_g}\left(\frac{\partial\theta_{gH}}{\partial p} - \frac{\partial\theta_{gW}}{\partial p}\right) \quad . \quad . \quad . \quad (8)$$

Now, $\theta_g = \tan^{-1} v_g/u_g$

Thus, $\dfrac{\partial\theta_g}{\partial p} = V_g^{-2}\left(u_g\dfrac{\partial v_g}{\partial p} - v_g\dfrac{\partial u_g}{\partial p}\right)$

Now, from the thermal wind equations for isobaric surfaces,

$$\frac{\partial u_g}{\partial p} = \frac{R}{lp}\left(\frac{\partial T}{\partial y}\right)_p, \quad \frac{\partial v_g}{\partial p} = -\frac{R}{lp}\left(\frac{\partial T}{\partial x}\right)_p$$

Hence, $$\frac{\partial\theta_g}{\partial p} = -\frac{R}{lp\,V_g^2}\left[u_g\left(\frac{\partial T}{\partial x}\right)_p + v_g\left(\frac{\partial T}{\partial y}\right)_p\right]$$

where R represents the gas constant for dry air. This expression gives, since only $(\partial T/\partial x)_p$ is discontinuous,

$$\frac{\partial\theta_{gH}}{\partial p} - \frac{\partial\theta_{gW}}{\partial p} = \frac{R\,u_g}{lp\,V_g^2}(\beta_H' - \beta_W') \quad . \quad . \quad . \quad (9)$$

From Eqs. (8) and (9), $$\kappa_{pH} - \kappa_{pW} = \frac{m_p R\,u_g^2}{lp\,V_g^3}(\beta_H' - \beta_W') \quad . \quad . \quad . \quad (10)$$

In this expression all terms on the right are positive and since κ_p is positive for cyclonic curvature it follows that at the front the hyperbaroclinic zone contours will have *greater cyclonic curvature* than the contours in the warm air or in the cold air proper. Thus the trough associated with a front will be found not at the conventional frontal position but on the cold-air side of this line in the hyperbaroclinic zone. This fact explains the tendency, prevalent among meteorologists in training, to retard the true motion of a surface cold-front when locating it on a new synoptic chart and to advance a surface warm-front beyond its true position in order to ' fit ' the front into the position of the sharpest pressure trough. It is the author's experience that, if surface fronts are properly placed and if pressure data are drawn in carefully, curvature discontinuity and orientation continuity of isobars will follow quite logically. This is also true at upper levels and indeed it would appear that many meteorologists have lost faith in the existence of fronts at upper levels since they could not legitimately show an angular trough in the height contours at their frontal positions, particularly in the case of warm fronts. Since cold fronts have frequently a very steep slope in the surface layers, and a correspondingly very narrow frontal zone, surface isobars will show a discontinuity very similar to a V trough as Eq. (10) indicates. This effect has also been described by Palmén (1946).

The magnitude of the discontinuities in curvature to be expected can be established by some numerical examples, based in part on the horizontal-temperature-gradient discontinuities used earlier. Consider first the case of a cold front. Assume $\theta_g = 45°$, $V_g = 15$ m/sec, $m_p = 2$ mb/mi ($\tan\theta \simeq 2 \times 10^{-2}$), $\beta_H' - \beta_W' = 8{\cdot}4°\text{C}/100$ mi, 45°N. latitude, $p = 500$ mb, and $\kappa_{pW} = 0$ (*i.e.*, straight contours in the warm air). Introducing $R_p = \kappa_p^{-1}$ for the radius of curvature, and using Eq. (10),

$$R_{pH} = 52 \text{ mi}$$

Thus a sharp cold front will show a very marked curvature discontinuity.

At a warm front assume the same values as above, except for $m_p = 0{\cdot}5$ mb/mi and $\beta_H' - \beta_W' = 3{\cdot}2°\text{C}/100$ mi. In this case the use of Eq. (10) gives :

$$R_{pH} = 540 \text{ mi}$$

Thus for normal warm fronts the curvature discontinuity is quite weak, representing only a small perturbation of the height contours. Inspection of Eq. (10) reveals that the curvature discontinuity will be a maximum for a steep frontal slope, a strong temperature-gradient discontinuity, a 90° angle between contours and the front, and a small geostrophic wind. The reverse of these criteria leads to a weak curvature discontinuity. Thus in general the discontinuity will be much more clearly evident at cold fronts, as demonstrated above.

Actually it is the isotherms, rather than the isobars, that change direction at a front in a discontinuous manner. If the angle between an isotherm and the front, θ_T, is defined as in Fig. 1(a) it follows that :

$$\tan\theta_T = -\left(\frac{dx}{dy}\right)_T = \frac{\partial T}{\partial y} \Big/ \frac{\partial T}{\partial x}$$

At the front $\partial T_W/\partial y = \partial T_H/\partial y$, but $\partial T_W/\partial x \neq \partial T_H/\partial x$. It follows that there is a change in the direction of the isotherms at a front such that the isotherms are more nearly parallel to the front in the hyperbaroclinic zone than in any other zone. However, since the isotherms in the air masses near the fronts are frequently nearly parallel to the front, it is difficult to construct the isotherm field from observations alone (considering their random errors) to show the true effect. Nevertheless the above criteria will be of value in obtaining the spacing and orientation of isotherms in the vicinity of fronts and care must be taken to avoid the violation of these principles (and the curvature principle) when a so-called ' synoptic smoothing ' of the data on upper-level charts is undertaken.

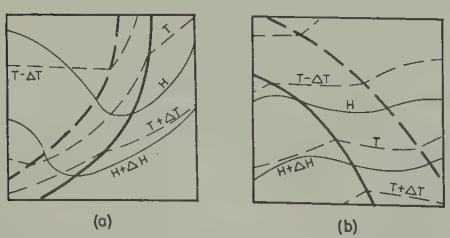

Figure 2. Height contours and isotherms at fronts ; (a) cold front, (b) warm front.

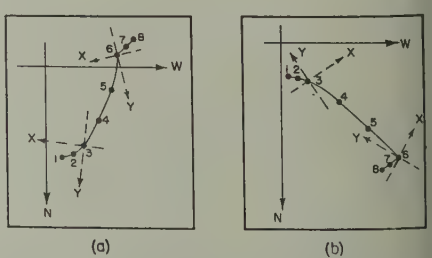

Figure 3. Schematic wind hodographs through frontal surfaces.

Some schematic examples of the correct construction of isotherms and height contours are shown in Fig. 2, (a) for a cold front and (b) for a warm front. The heavy full line represents the conventional front in both cases, the heavy dashed line the rear of the hyperbaroclinic zone. Full thin lines represent height contours of some isobaric surface and dashed thin lines represent the isotherms in that surface. For simplicity, the angles between isotherms and fronts are much greater than normal.

4. Principles of upper-level frontal analysis

(*a*) *Analysis of radiosonde ascents*

Preceding paragraphs have treated the construction of isobars and isotherms in the vicinity of fronts. These techniques will not increase the accuracy of upper-level charts unless the frontal positions are chosen in accordance with the same definition of a frontal discontinuity. The chief information on the location of fronts aloft can be obtained from the careful analysis of all available plotted radiosonde ascents and wind hodographs, and such analyses will be discussed here with respect to the identification of the top and the bottom of the hyperbaroclinic zone from appropriate atmospheric soundings.

Since both such surfaces must appear in the vertical as lapse-rate discontinuities the fundamental criteria of Bjerknes (1932) remain unaltered. The true frontal surface is chosen as the level where the lapse rate changes from a very small value below (often quasi-isothermal) to a less stable, or near normal, value above. The under-surface of the hyperbaroclinic zone represents the level of transition from a normal (and frequently steep) cold-air lapse-rate to a very stable lapse rate above. At times the discontinuity may appear to take place over a layer, in discrete steps, as a result of a finite number of reported levels in the sounding. When this occurs the level of maximum lapse-rate change should be chosen. In doubtful cases the moisture values may assist in delineating the zones of the pure air masses, or possibly a temperature comparison with a previous ascent or with an ascent at a nearby station may be useful. It must be realized that a weak vertical discontinuity may frequently be associated with a reasonably strong horizontal discontinuity (for steep fronts) and therefore frontolysis can only be assumed to have taken place when *both* vertical and horizontal discontinuities are no longer evident.

(*b*) *Analysis of upper-wind data*

Valuable information on the heights and approximate orientations of frontal surfaces can be obtained from upper-wind soundings plotted on polar-coordinate paper, *i.e.*, from wind hodographs. Qualitatively it is evident from the thermal-wind equations that since the maximum temperature gradient across a front is found in the hyperbaroclinic zone the maximum rate of vertical wind shear will also occur in that zone. Similarly, since this maximum temperature gradient is directed almost normal to the front the direction of the wind-shear vector in that layer will give the approximate orientation of the front. Such concepts have been presented by Penner (1947) and others. The quantitative application of these criteria depends to a large extent on the validity of the geostrophic assumption inherent in the thermal-wind equations.

Although the true wind speed, V, is frequently quite different from that of the geostrophic wind, V_g, the true wind direction, θ, is almost always a good approximation to that of the geostrophic wind, θ_g (see Godson 1950c). Thus in Eq. (9) θ_g can be replaced by θ and u_g/V_g by u/V.

Thus,
$$\frac{\partial \theta_H}{\partial p} - \frac{\partial \theta_W}{\partial p} = \frac{R}{lp}\frac{u}{V}\frac{\beta_H' - \beta_W'}{V_g} \quad . \quad . \quad . \quad (11)$$

Similarly,
$$\frac{\partial \theta_H}{\partial p} - \frac{\partial \theta_c}{\partial p} = \frac{R}{lp}\frac{u}{V}\frac{\beta_H' - \beta_c'}{V_g} \quad . \quad . \quad . \quad (12)$$

The magnitude of this discontinuity can be evaluated from the data assumed earlier for typical cold and warm fronts. Assume further that $\partial \theta_W/\partial p = 0$. Thus at the cold front, $\partial \theta_H/\partial p = 80°/100$ mb, which is roughly equivalent to $20°/1{,}000$ ft. At the warm front, $\partial \theta_H/\partial p = 32°/100$ mb, which is roughly equivalent to $8°/1{,}000$ ft. In the case of cold fronts, at least, the discontinuity in $\partial \theta/\partial p$ (or $\partial \theta/\partial z$) is quite well marked at the boundaries of the hyperbaroclinic zone, provided that the ratio u/V is sufficiently large.

If the wind at a front blows almost parallel to the front, u/V will have a small value and the above criterion is no longer useful. The thermal-wind equations give for the variation of geostrophic wind speed with height,

$$\frac{\partial V_g}{\partial p} = \frac{R}{l_p V_g}\left[u_g\left(\frac{\partial T}{\partial y}\right)_p - v_g\left(\frac{\partial T}{\partial x}\right)_p\right]$$

Hence,
$$\frac{\partial V_{gH}}{\partial p} - \frac{\partial V_{gW}}{\partial p} = \frac{R}{lp}\frac{v}{V}(\beta_H' - \beta_W') \quad . \quad . \quad . \quad (13)$$

Similarly,
$$\frac{\partial V_{gH}}{\partial p} - \frac{\partial V_{gC}}{\partial p} = \frac{R}{lp}\frac{v}{V}(\beta_H' - \beta_C') \quad . \quad . \quad . \quad (14)$$

These two equations are only valid for observed winds if the ratio V/V_g changes with pressure at approximately the same rate in the various zones. Even when this is not strictly true the general deductions from Eqs. (13) and (14) will not be affected, except in rather unusual cases. The magnitudes of the discontinuities implied by Eqs. (13) and (14) can be deduced from the data previously used, assuming for the sake of simplicity that $\partial V_{gW}/\partial p = 0$. Thus for the cold front, $\partial V_{gH}/\partial p = 46$ mi hr^{-1} (100 mb)$^{-1}$, which is roughly equivalent to 12 mi hr^{-1} (1,000 ft)$^{-1}$. Thus for the warm front, $\partial V_{gH}/\partial p = 18$ mi hr^{-1} (100 mb)$^{-1}$, which is roughly equivalent to 4 mi hr^{-1} (1,000 ft)$^{-1}$. In the case of sharp cold fronts the discontinuity will again be well marked if the ratio v/V is not too small. The hodograph analysis of shallow fronts will in general be difficult, and clear-cut discontinuities in the vertical wind shear will seldom be experienced.

If the relations derived above are to be used quantitatively it is necessary to know the orientation of the front. This can be obtained when it is recalled that $(\partial T/\partial y)_p$ is continuous at the front and hence $\partial u_g/\partial p$ must also be continuous. The axes can be chosen to fulfil this condition in a manner which will now be described.

Fig. 3 (a) and (b), shows two schematic wind hodographs. The end of the wind vector is represented by a solid circle at each of 8 successive levels (pressure or

height) numbered vertically upwards. The wind hodograph is represented by a full line and the X and Y axes by dashed lines with their origins at the points of discontinuity of the wind soundings.

In Fig. 3 (a) the wind changes with height from NNW. to WSW. The shears in the layers 1-3 and 6-8 are relatively small while the shear in the layer 3-6 is relatively large and this layer may be identified as the hyperbaroclinic zone. Consider first the under-surface of this zone which intersects the sounding at level 3. The X axis is to be chosen as pointing towards the cold air, *i.e.*, roughly to the left of the strong shear from 3 to 6. The Y axis, parallel to the front, must be chosen so that $\partial u_g/\partial p$ is continuous, *i.e.*, so that $\Delta u_{2-3} \simeq \Delta u_{3-4}$ or so that the perpendiculars on the Y axis from points 2 and 4 have the same length but lie on opposite sides of the axis. It can be seen that this construction has been followed on the diagrams.

Referring to wind changes in the vicinity of level 3 (in both Figs. (a) and (b)) it is clear that the relations in Eqs. (12) and (14) are satisfied qualitatively ; in the vicinity of level 6, that of the conventional front or the upper surface of the hyperbaroclinic zone, it is clear that the relations in Eqs. (11) and (13) are satisfied qualitatively.

These schematic diagrams have demonstrated the use of techniques whereby the two levels of the hyperbaroclinic zone may be identified from a wind hodograph, as well as the orientation of the intersection of each surface with isobaric surfaces. By using upper-wind data in this manner, and radiosonde data as described previously, it should be possible to obtain frontal positions aloft almost as accurately as at the earth's surface, if analyses of vertical temperature- and wind-soundings are coordinated properly.

It may be noted that the analysis of a simultaneous temperature- and wind-sounding provides a check on the two frontal levels to be identified, gives the orientation of both boundary surfaces, and permits a computation of the slope of each surface. Thus Eqs. (11)-(14) can be solved for $(\beta_H' - \beta_W')$ and $(\beta_H' - \beta_C')$, the radiosonde data give directly $(\gamma_W' - \gamma_H')$ and $(\gamma_C' - \gamma_H')$, and Eqs. (4) and (5) can then be solved for m_p at each surface. A simpler relation for the determination of $\Delta\beta'$ may be obtained in terms of the discontinuity in $\partial v_g/\partial p$ at the front. From the appropriate thermal wind equation it follows directly that :

$$\frac{\partial v_{gH}}{\partial p} - \frac{\partial v_{gW}}{\partial p} = \frac{R}{lp}(\beta_H' - \beta_W') \quad . \quad . \quad . \quad (15)$$

$$\frac{\partial v_{gH}}{\partial p} - \frac{\partial v_{gC}}{\partial p} = \frac{R}{lp}(\beta_H' - \beta_C') \quad . \quad . \quad . \quad (16)$$

5. Vertical motions and frontal speeds

It will be clear that the presence of clouds does not necessarily imply ascending currents, nor clear skies subsidence. Weather changes, following the general air motion, may at times be associated with the reverse of the expected conditions, and the forecasting of these weather developments presents a very vital problem. The evaluation of vertical velocities for these purposes can most effectively be performed with quasi-substantial surfaces, such as frontal surfaces, isen-

tropic surfaces, *etc.* The chief merits of isentropic analysis were the delineation of vertical motions and moisture tongues, although some clues for development could at times be obtained. Moisture studies on isobaric surfaces are equally valuable if the vertical component of air motion can be assessed.

Valuable deductions may be made concerning the speed of a front, the slope of a front, and the magnitude of vertical air motions by postulating that an air parcel on a frontal surface must remain on a frontal surface. The relation for the vertical velocity, with this assumption, has been given by Gião (1929) and Petterssen (1933) as :

$$w = (u - u_F) \tan \theta \quad . \quad . \quad . \quad . \quad . \quad (17)$$

where w signifies the vertical velocity, u_F the speed of the front (normal to itself), and u and tan θ are as defined previously.

In terms of pressure as the vertical co-ordinate w is replaced by dp/dt, which is the significant property as far as approach to condensation is concerned, and tan θ is replaced by m_p, giving (with the necessary change of sign)

$$dp/dt = m_p (u_F - u) \quad . \quad . \quad . \quad . \quad . \quad (18)$$

Thus if the component of the air motion towards the cold air mass exceeds the frontal-layer speed in that direction, ascent will occur ; if it is less, subsidence will occur. u_F and m_p can be found from frontal contour charts, the former from successive charts, *i.e.*, at a 6- or 12-hourly interval.

Eqs. (17) and (18) define the vertical motion only in a mathematical sense, not in a physical sense. The so-called ' frontal-lifting ' process does not operate as such since no independent forces act to move the front ; its motion is simply that of an array of air parcels at the boundary layer of an air mass, constituting essentially a quasi-substantial surface. The vertical motion is specified by the integrated divergence below the level in question and in turn, with the horizontal motion, determines the frontal speed.

Eq. (18) may be applied qualitatively to consider the variation of dp/dt in the vertical through the hyperbaroclinic zone, since the relation applies to both the upper and lower frontal boundary surfaces as observed at a given station. It may be assumed for simplicity that the frontal orientation does not vary appreciably in the vertical. Using the synoptic situation to be outlined in the next section, a study was made of the change of frontal orientation through the hyperbaroclinic zone. The mean change for 38 well-defined cases was 18°, which is a relatively small value in view of the uncertainties in the evaluation of frontal orientations. The mean vertical thickness of the frontal layer was 100 mb. Over this depth the mean numerical change of u was 14 mi/hr, and of u_F, 5 mi/hr. The data showed also that the relative change with pressure of m_p was considerably less than that of dp/dt. Hence from a qualitative point of view valuable deductions may be made from a single hodograph by assuming that the chief variations in the vertical are those of u and dp/dt.

As Sutcliffe (1939) has pointed out, dp/dt will tend to have the same sign throughout the troposphere, with a maximum numerical value at the level of non-divergence, in the vicinity of 600 mb. Over level terrain dp/dt is virtually zero at

the ground and generally increases numerically upwards in the lower troposphere. It follows that $(u - u_F)$ will be numerically smaller, on the average, at the lower frontal surface than at the upper. The 38 cases cited above gave absolute means of 3 and 19 mi/hr, respectively, for this quantity. Over western North America the terrain is far from level which will explain in part the relatively large mean value of $(u - u_F)$ at the under-surface (observed even when the frontal surface was low), since a majority of the analysed hodographs referred to that area. In a majority of the cases dp/dt had the same sign as $-\frac{\partial}{\partial p}\frac{dp}{dt}$ below 600 mb and the opposite sign above, as theory would predict. Using these considerations it is possible to deduce the sign and change of dp/dt through the frontal layer from a single hodograph, and even to arrive at a better approximation to u_F then u at the lower level.

As examples, the hodographs in Fig. 3 may be considered briefly. In Fig. 3(a), u at level 6 is negative and numerically larger than u at level 3. From Eq. (18), $\frac{\partial}{\partial p}\frac{dp}{dt}$ is probably negative, corresponding from the above analysis to slight subsidence at the lower level and greater subsidence in the warm air. Hence at the lower level using Eq. (18), $(u_F - u)$ is positive, *i.e.*, u_F is slightly less negative than u at level 3. Thus the front will be a cold front, probably without significant weather, except for cumulus resulting from the cold air advection in the cold air mass.

In Fig. 3(b), u at level 3 is almost zero and at level 6 is definitely negative. Again subsidence increasing with height would be anticipated and at the lower level $(u_F - u)$ would accordingly be positive, corresponding to a slow motion as a warm front. If the motion and slope of the front at various levels were known, the vertical motions could be computed directly from Eq. (18). If it is assumed that a mean frontal motion for 6 or 12 hr is reasonably representative of the value at the end of the period, it follows that Eq. (18) will yield instantaneous values of dp/dt. In view of the uncertainties in the terms involved, this is probably a reasonable assumption.

In the concluding section of this paper, a synoptic situation is presented with illustrative examples of various phases of the suggested techniques and principles of this paper. The true significance of the individual upper-air ascents cannot be grasped, however, until the general state of the atmosphere has been described. Hence the overall picture is examined first, and the opportunity is taken of extending the remarks for this specific case to the more general problems encountered in frontal analysis.

6. Synoptic example of analysis technique

The synoptic situation over North America and adjacent ocean areas for 4 April 1950 was chosen as an example of the principles enumerated in previous sections. The synoptic situation itself was rather simple and the analysis at the surface and aloft more clear-cut than is usual.

Fig. 4 presents the surface chart, for 4 April, at 1530 GMT. Isobars are drawn at intervals of 4 mb and fronts are represented in the conventional manner. The air mass to the south of the front is tropical and the air to the north is a relatively cold maritime polar air mass which merges into an air mass of arctic origin, modified

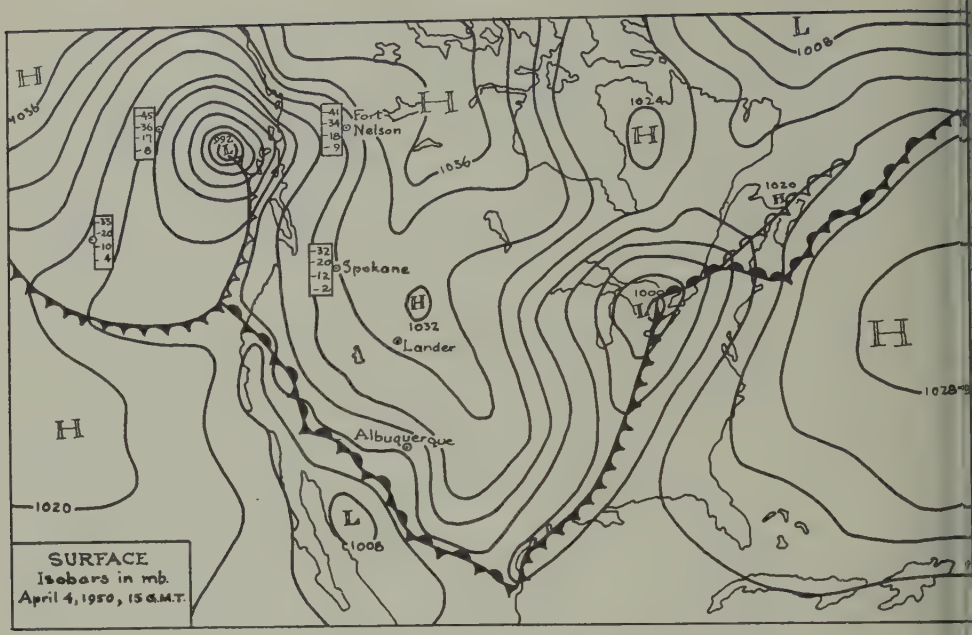

Figure 4. Surface synoptic chart for 4 April 1950, 1530 GMT.

in the lower levels, *i.e.*, maritime arctic air (Godson 1950d). Thus the frontal system represents both the arctic and polar fronts and as a result of this extreme in-phase relation, cyclones on the front should develop. This process is just commencing for the eastern wave ; the western wave, on the other hand, is well occluded.

The occluded wave on the west coast is associated with an upper cold front, but *not* with a surface or ' occluded ' front. A detailed analysis of upper-air data in this region reveals that this model completely and adequately fits at least 95 per cent of all occluded frontal waves crossing the west coast of North America. To the north of the wave crest on the polar front is found no surface front but instead an upper cold front in a pressure trough, corresponding to a sloping valley of tropical air aloft. This ' valley ' structure is borne out by the frontal contour chart of Fig. 5. On this chart are shown the positions of the polar front for each 100 mb surface from 1,000 to 300 mb. This chart was constructed very carefully from all radiosonde and upper-wind data, maintaining continuity with previous individual ascents as well as frontal-contour analyses. The heavy dashed line off the west coast represents the upper cold front, which can also be described as the locus of the wave crests at upper levels.

In order to demonstrate that no surface front exists beneath the upper cold front, representative upper air temperatures (in °C) have been entered in boxes on Fig. 4 for four radiosonde stations : Fort Nelson, B.C. ; Spokane, Mont. ; and two Pacific ships. From bottom to top the temperatures refer to the pressure levels of 850, 700, 500, and 400 mb, respectively. The two southern stations are in cold air at 850 and 700 mb, and in tropical air at 500 and 400 mb ; the two northern stations are in maritime arctic air to above 400 mb (*cf.* Fig. 5). Comparisons between

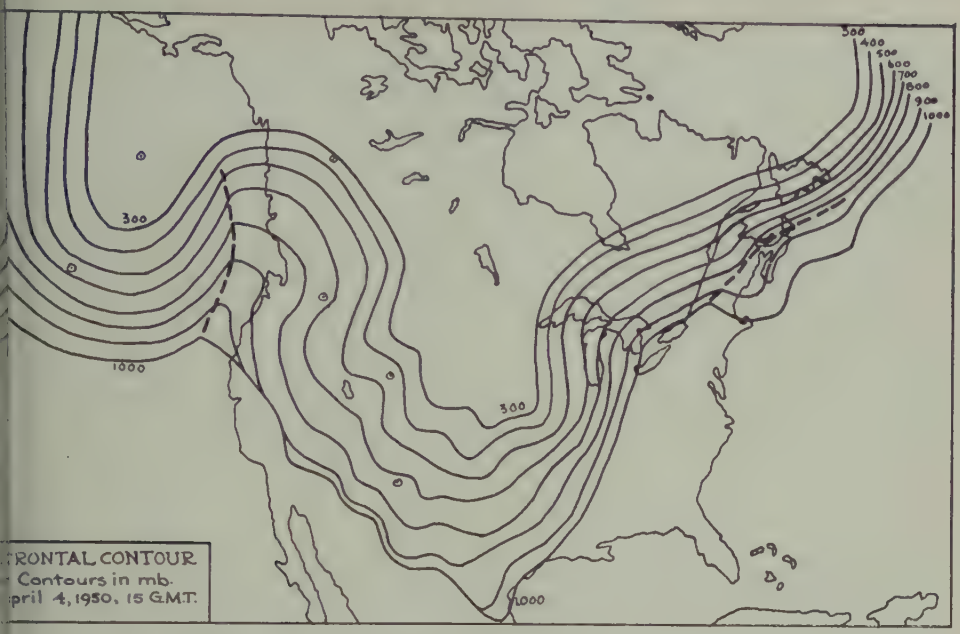

Figure 5. Frontal-contour chart for 4 April 1950, 1530 GMT.

mperatures for these two pairs of stations reveal that the air below the polar ontal surface is essentially the same to the west and east of the upper cold front. oreover, the weather along this latter line is due principally to the tropical air cending the frontal surface. Upper cold fronts can be located chiefly by their essure trough (especially well-marked in the pressure-tendency field) and by the earing of middle cloud subsequent to the passage of the upper cold front.

In other parts of North America frontal waves at times have a more complex ucture than that illustrated here. Frequently a third air mass will be involved rctic air) in addition to the tropical and maritime polar air masses separated by e polar front. This air mass (maritime or continental arctic, often referred to as ntinental polar especially when modified by a trajectory southwards) is normally parated from maritime polar air by the arctic front. Almost invariably a wave l the arctic front is in phase with a wave on the polar front. In western or north-estern Canada each upper cold front sets off a wave on the arctic front, generally st to the east of the continental divide (Carpenter 1940). When this occurs, arctic is brought into the circulation around the polar-front cyclone and the linkage tween the two frontal systems becomes more apparent.

This process is illustrated by the schematic synoptic charts of Fig. 6, on which e shown fronts, isobars, and air-mass designations. In (a) the polar upper cold ont is approaching the arctic front. In (b) it has set off a wave on the arctic front. s a result, arctic air moves rapidly southward to the west of the surface centre. s the frontal systems move, three alternative developments may ensue. In the st case, the frontal systems may slowly approach one another, either amalgamating mpletely (as in Fig. 4) or reaching an equilibrium separation of several hundred

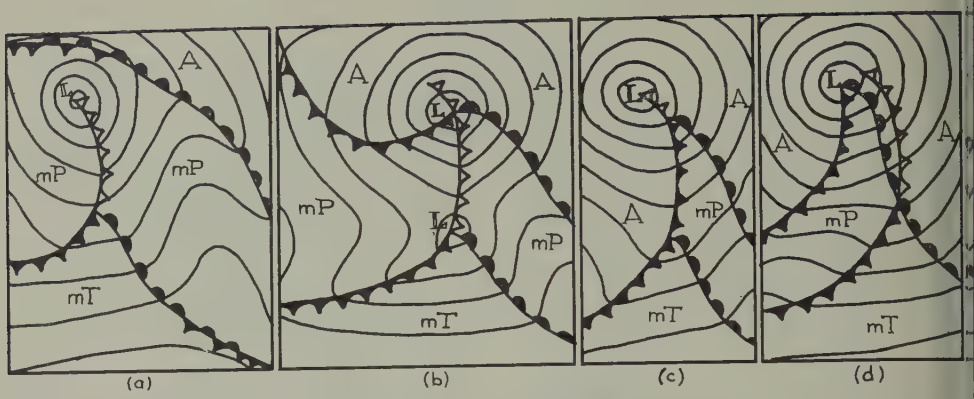

Figure 6. Schematic surface synoptic charts illustrating possible interactions between the polar and arctic fronts.

miles, giving a close in-phase double frontal structure. In the second case, the cold fronts may amalgamate while the warm fronts remain separate (Fig. 6(c)). Part of the surface arctic cold front will remain to the north of the polar wave crest ; this will in general virtually coincide with the position of the polar upper cold front. In the third case, the warm fronts may amalgamate while the cold fronts remain separate (Fig. 6(d)). When this occurs the polar wave generally outstrips the arctic wave, as illustrated. It will be noted that only in Fig. 6(c) would the occlusion designation be at all useful ; even in this case it is generally more accurate to show the arctic cold front as slightly to the rear of the polar cold front, with the upper cold front extending toward the centre of low pressure.

If the occlusion process is concerned with only a single cold air mass (as in Fig. 6(a)), no surface front can exist in the trough northward from the wave crest if a front is defined in terms of a discontinuity zone between adjacent air masses. As a result of differences in trajectories in the lowest layers, surface temperatures will often be slightly cooler to the rear of the pressure trough. Conversely, owing to differences in cloudiness, surface temperatures may at times be lower in advance of the upper cold front. In both these cases, temperatures at upper levels (say, 900-700 mb) will reveal that no significant horizontal temperature-gradient discontinuity exists below the polar frontal surface. If three air masses are present, there must be two frontal systems ; for each one the problem reduces, separately, to a two-air-mass situation as above.

With reference to Fig. 4, one further feature of the analysis deserves special comment – the upper warm and quasi-stationary front crossing the maritime provinces. As described by Crocker, Godson, and Penner (1947), this represents a line along which the frontal surface steepens. This structure is well illustrated by the frontal-contour chart (Fig. 5) on which this upper front has been reproduced as a broken line. The front is very shallow to the south of the upper front and very steep to the north. As would be expected, the major trough was found along the upper, and not the surface, front. Nevertheless, no surface front was present in the trough, a fact borne out by the surface data in that region.

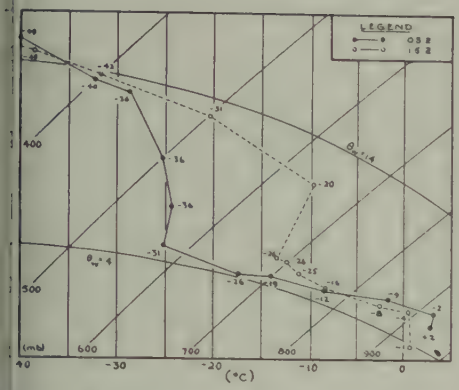

Figure 7. Tephigrams for Spokane, Mont., for 4 April 1950 : ●——●, 0330 GMT ; o - - - - o, 1530 GMT (isotherms every 5°C, isobars every 100 mb, dew point temperatures entered in whole °C).

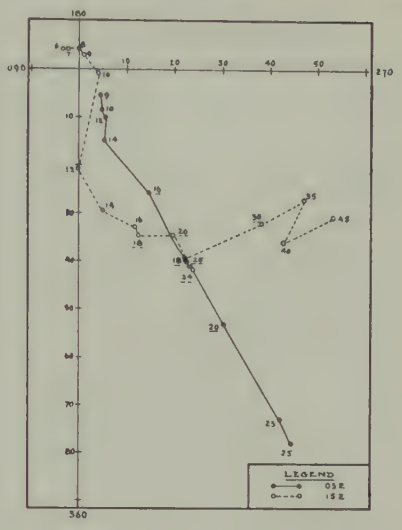

Figure 8. Hodographs for Spokane, Mont., for 4 April 1950 : ●——●, 0330 GMT ; o - - - - o, 1530 GMT (wind speed in kt along cardinal directions, heights in thousands of ft, entered beside points).

Some of the ascents on which the three-dimensional structure was based will now be presented. In Fig. 7 are shown tephigrams for Spokane, Mont., for 0330 and 1530 GMT. This station (and others mentioned later) will be found marked on the surface chart, Fig. 4. The figures beside the points on these curves give dew point in °C. Two pseudo-adiabats are shown ; dry adiabats (not shown) are perpendicular to the isotherms. The corresponding upper-wind hodographs, at 0330 and 1530 GMT, are shown in Fig. 8. The figures beside the points on these curves give the heights in thousands of ft. The end points of the wind vectors at successive levels are joined by straight lines. Wind directions (as coded in wind reports) are indicated along the cardinal directions. These orthogonal axes are calibrated for wind speed in kt.

At 0330 GMT, the Spokane radiosonde ascent shows maritime arctic air (or extremely cold maritime polar air) to 570 mb, a hyperbaroclinic layer to 430 mb, and tropical air aloft. The radiowind ascent shows weak horizontal temperature gradients to 14,000 ft (595 mb), a hyperbaroclinic zone to roughly 23,000 ft (400 mb), and weaker temperature gradients aloft. Since the upper winds are interpolated from two-minute average winds, the wind-shear discontinuity above and below the hyperbaroclinic zone will be weakened and this zone will appear to be a somewhat deeper layer then is actually the case. This fact must be kept in mind during hodograph analysis. The orientation of the upper discontinuity (or true frontal) surface is approximately 330°, when moving along the front with cold air to the left, with no wind component perpendicular to the front. Since the front is moving as a warm front, the warm air must be subsiding (at 0630 GMT, skies were clear at Spokane). The orientation of the lower surface of discontinuity is approximately 300°, with a down-

slope wind component of about 10 kt. This suggests stronger subsidence at the 570 mb level than at the 430 mb level, which is in agreement with a level of non-divergence in the vicinity of 600 mb. The presence of subsidence is further demonstrated by the dry inversion to 550 mb. In the tropical air the moisture content is relatively high, despite the subsidence, suggesting that the air had recently been saturated.

At 1530 GMT, the Spokane radiosonde ascent (Fig. 7) shows that the base of the hyperbaroclinic layer has lowered to 685 mb and the top to 645 mb. The radiowind ascent (Fig. 8) suggests that the limits of the hyperbaroclinic zone are at approximately 10,000 ft (700 mb) and 12,000 ft (650 mb). Once again the automatic wind-smoothing produces an artificial deepening of the frontal layer. The orientation of the lower surface is approximately 30°, and of the upper surface 40° (with the cold air mass to the left). This can be seen to check with the frontal contour chart (Fig. 5), prepared from many observations in this area. The upslope wind component is 4 kt at the lower surface and 13 kt at the upper surface. If the upper and lower surfaces were moving at the same speed, the above data might be interpreted as ascent increasing with height. However, an inspection of Fig. 7 reveals that the upper surface has moved much more rapidly than the lower surface, unless the slope varied greatly with height, which was not the case. This may be deduced from Figs. 7 and 8, since for the 1530 GMT ascents the vertical and horizontal discontinuities are very similar for both boundary surfaces. The use of Eq. (18) reveals that at the lower surface $dp/dt = 8$ mb/3 hr, at the upper surface $dp/dt = 15$ mb/3 hr, again consistent with a level of non-divergence in the vicinity of 600 mb. A single-station qualitative analysis of the hodograph only, would have given an incorrect result in this case. It may be noted that the cold air at 700 mb has become warmer and drier in the 12-hr period between ascents. The rate of warm-air advection (from Fig. 8) is very slow so that the above rate of subsidence ($+$ 32 mb/12 hr) can easily be justified, at least qualitatively. The presence of subsidence in the tropical air is borne out by a study of the weather along the 500 mb trajectory of the air over Spokane at 1530 GMT. At 0630 GMT this air was over south-central British Columbia and the reports showed overcast cirrostratus with scattered stratocumulus. At 1830 GMT this air was to the SSE. of Spokane and the sky was coded as $< 5/10$ cirrostratus (not increasing). At 0630 GMT, 5 April, the sky was clear at the 500 mb trajectory point.

As the vertical depth of the frontal hyperbaroclinic layer decreased, from 0330 to 1530 GMT, the front became shallower, and at 1530 GMT the slope of both boundary surfaces was roughly 2 mb/mi, *i.e.*, 1/250. The vertical discontinuity (on the tephigram curves of Fig. 7) increased at both surfaces ; the horizontal discontinuity (on the hodograph curves of Fig. 8) decreased at both surfaces, as predicted in Section 3(a).

In Fig. 9 are shown tephigrams for Lander, Wyo., and Albuquerque, New Mex., for 4 April 1950, at 1530 GMT. The locations of these stations are shown on the surface map, Fig. 4. The corresponding hodographs are shown in Fig. 10. The letter M beside certain points on Fig. 9 indicates that the dew point was missing, most likely due to the motor-boating phenomenon, with relative humidities too low to give a definable signal.

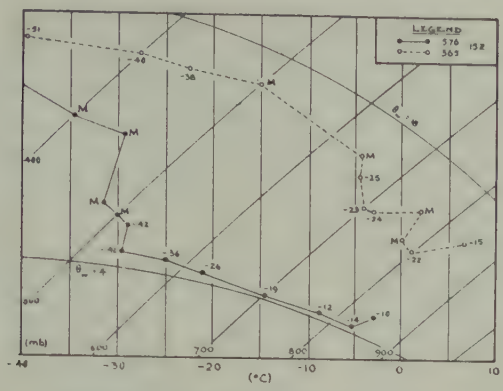

Figure 9. Tephigrams for 4 April 1950 : ●———●, Lander, Wyo. ; o - - - - o, Albuquerque, New Mex. (see Fig. 7 for legend).

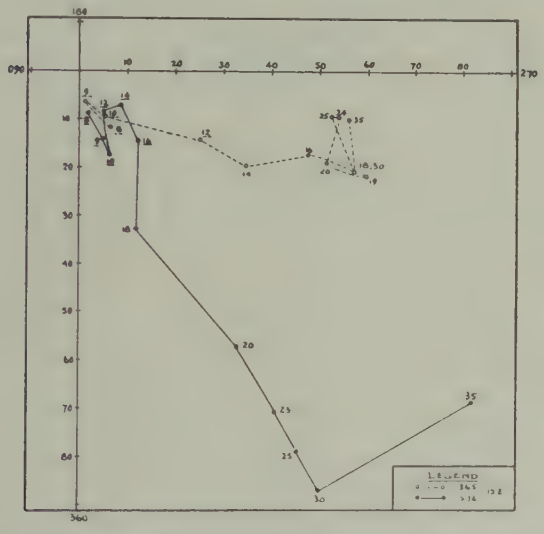

Figure 10. Hodographs for 4 April 1950 : ●———●, Lander, Wyo. ; o - - - - o, Albuquerque, New Mex. (see Fig. 8 for legend).

At 1530 GMT the Lander radiosonde ascent shows maritime arctic air to 530 mb, a hyperbaroclinic layer to 445 mb, and tropical air (or at least very warm maritime polar air) aloft. Thus the polar front is at 445 mb. The radiowind ascent suggests the presence of a hyperbaroclinic zone from about 16,000 ft (550 mb) to about 20,000 ft (460 mb). The corresponding orientations are 20° and 320° (with the cold air mass to the left). At the lower boundary surface the wind has an upslope component of 17 kt ; at the upper boundary surface the wind has a downslope component of 18 kt. Using observed values for the speed and slope of these two surfaces the resulting vertical motions are : 530 mb, $dp/dt = +16$ mb/3 hr ; at 445 mb, $dp/dt = +74$ mb/3 hr. The weather along the 500 mb trajectory of the air over Lander at 1530 GMT was as follows : 0630 GMT, scattered altocumulus and broken stratocumulus ; 1830 GMT, scattered stratocumulus ; and 0630 GMT, 5 April, clear.

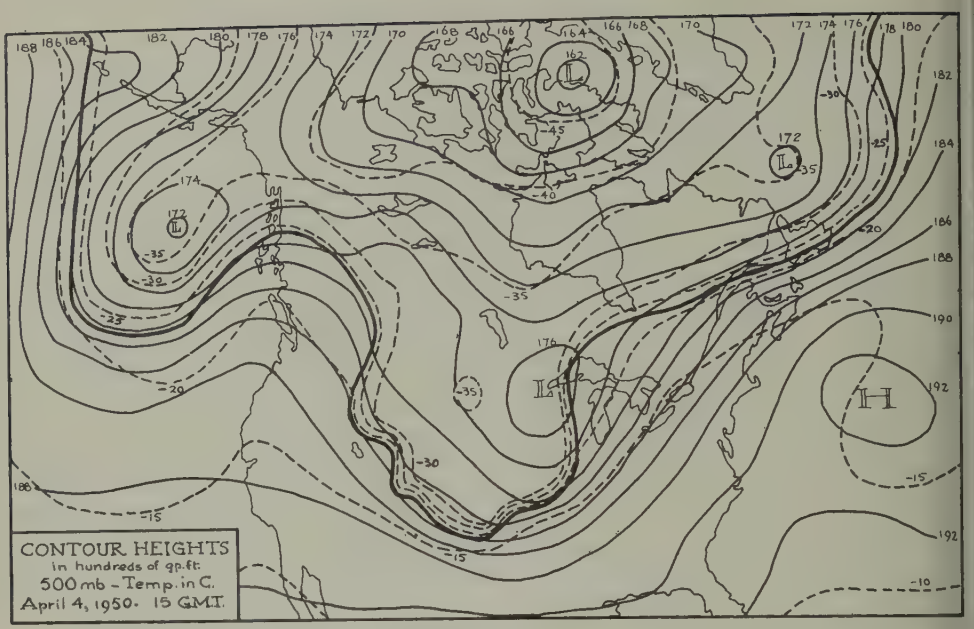

Figure 11. 500 mb chart for 4 April 1950, at 1530 GMT. The polar front is shown as a heavy full line ; contours are shown as thin full lines and are labelled in hundreds of gp ft ; isotherms are shown as dashed lines and are labelled in °C.

The analysis of the Albuquerque ascents is slightly more difficult. Below 700 mb the air mass is cold maritime polar air, probably with an arctic history (judging by the dew points). This air is being modified by subsidence (dry inversion 770 to 760 mb) and absorption of solar radiation (superadiabatic lapse rate to 800 mb). Above 640 mb the air mass is a tropical one. It is warmer at upper levels than the warm air over Lander, which would be expected. As the front lowers over a station, temperatures in the warm air (and cold air) slowly rise. The hodograph shows the hyperbaroclinic zone between 10,000 ft (705 mb) and 12,000 ft (650 mb), with orientations of 250° and 270°, respectively. The computed vertical motions were + 24 mb/3hr and + 36 mb/3hr, again in agreement with a level of non-divergence near the 600 mb level. The weather along the 500 mb trajectory of the air over Albuquerque at 1530 GMT was clear from 0630 GMT, 4 April, to 0630 GMT, 5 April.

The 500 mb chart corresponding to the synoptic situation studied is shown in Fig. 11. The polar front (heavy full line) was entered from the frontal-contour chart and the 500 mb chart was then constructed by differential analysis. Thickness lines were drawn with this chart placed over the frontal contour chart on a light-table. The behaviour of contours and isotherms can be seen to be in general agreement with the principles enunciated earlier. In general, the isotherms near the front are very nearly parallel to the front, which is a logical result of the quasi-homogeneous air-mass concept. The discontinuity in horizontal temperature gradient normal to the front is clearly evident, especially where the frontal surface is steep.

It is interesting to note the difference in behaviour at 500 mb of the occluded wave off the west coast and the developing wave in the Great Lakes region. In the latter case the 500 mb low is far from being thermally symmetrical ; in fact, the relation between the open-wave frontal system and the low-pressure area is not unlike that prevailing at the earth's surface (*cf.* Fig. 4).

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Note on eddy diffusion in the atmosphere between one and two kilometres

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Summary

Information on eddy diffusion in the atmosphere at heights between one and two kilometres has been obtained from an analysis of observations made on pairs of smoke puffs by using a camera obscura. The results are compared with a prediction of the Kolmogoroff similarity hypotheses.

1. Introduction

The application of the Kolmogoroff similarity hypotheses to observations of the small-scale components of turbulence in wind tunnels is now well known, and Batchelor (1950) has written on the possible application of the theory to problems of atmospheric and oceanic turbulence.

The simplicity and power of the hypotheses are such that any observational evidence as to the circumstances in which they may be used in the atmosphere is most valuable. The observations described by Durst (1948) have been analysed with this in mind.

2. Observations

The observations used have been carefully described by Durst (*loc. cit.*). Pairs of smoke puffs, released from an aircraft, were tracked by using a camera obscura and their co-ordinates relative to the (fixed) axes of the plotting table were noted immediately on release and subsequently at ten-second intervals.

Their co-ordinates were recorded to the nearest hundredth of an inch on the table and have been used at that nominal accuracy although the last figure is perhaps uncertain. On many occasions the motion of the puffs immediately on release appeared to differ from their subsequent motion, so that all observations of their motion during the first ten seconds after release have been ignored as being possibly affected by the passage of the aircraft. No other observation was discarded, but in a very few cases what appeared to be errors of whole inches or half inches on the table were noted and altered.

3. Analysis

Elsewhere in this *Journal* Batchelor (*loc. cit.*) has discussed the application of the Kolmogoroff similarity hypotheses to this type of observation, and one of the predicted results is :

$$F(t, l(0)) \equiv \frac{\overline{[l(t) - l(0)]^2}}{t} \simeq \frac{4}{3} c_1 (\epsilon l(0))^{2/3} t \quad . \quad . \quad . \quad (1)$$

provided,

$$t < t_1 = \alpha \epsilon^{-1/3} l(0)^{2/3} \quad . \quad . \quad . \quad . \quad (2)$$

Here, c_1 and α are universal constants of order unity, ϵ is the average energy dissipation per unit mass, $l(0)$ is the initial separation between the two puffs at time $t = 0$, $l(t)$ is the projection on to a horizontal plane of the distance between them t seconds later. The bar implies a mean value over many trials using the same initial separation with both puffs initially in the same horizontal plane.

The observations have been analysed in an attempt to check their consistency with this prediction.

TABLE I.

Date	l_0 cm $\times 10^3$	$F(t, l_0)$ cm^2 sec^{-3} $\times 10^3$; corresponding number of observations given in brackets					mean $F(t, l_0)/t$ cm^2 sec^{-3} $\times 10^3$	ϵ cm^2 sec^{-3}	t_1 sec $\times 10^2$	wind speed cm sec^{-1} $\times 10^2$
		$t = 10$	20	30	40	50				
7.10.46	42	21(38)	31(30)	43(27)	59(24)	77(21)	15	0·9	12	2·5
14.10.46	17	10(24)	15(18)	17(13)	21(8)	21(5)	5·4	0·5	8	4·5
	21	6(40)	9(35)	11(29)	15(24)	19(18)	3·8	0·2	13	4·5
21. 1.47	77	30(18)	43(15)	59(12)	77(9)	93(6)	20	0·8	19	0·5
3. 3.47	21	42(17)	3(15)	3(13)	2(11)	3(9)	—	—	—	4·0
14. 8.47	32	22(8)	16(7)	19(6)	22(5)	27(4)	5·9	0·3	15	3·6
	49	24(26)	27(22)	31(18)	43(14)	41(10)	10	0·4	17	3·6
4.10.46	15	8(24)	9(18)	9(14)	7(11)	7(9)				10
	19	17(37)	15(32)	13(26)	14(20)	17(15)				10
8.10.46	14	20(24)	18(19)	19(14)	26(11)	32(9)				8
	20	29(19)	34(16)	43(13)	43(10)	45(7)				8
10.10.46	38	17(12)	15(9)	17(6)	4(3)	2(2)				15
	51	27(13)	42(9)	37(5)	8(3)	10(2)				15
11.10.46	35	13(16)	6(13)	7(11)	8(8)	10(6)				12
	45	13(43)	16(38)	17(31)	20(26)	20(21)				12
	54	16(19)	9(16)	16(13)	20(10)	27(7)				12
18.10.46	4	27(17)	21(12)	25(7)	52(3)	62(2)				9
	8	49(15)	98(9)	117(6)	129(2)	—				9
17. 1.47	38	29(19)	46(14)	53(9)	48(4)	44(2)				17
	55	34(14)	34(10)	26(6)	17(3)	18(2)				17

It was assumed that the two puffs were released at the same height (given by the pressure altimeter in the aircraft) and remained at that height. For each day of observation values were computed of

$$F(t, l_0) \equiv \frac{\overline{[l(t) - l(0)]^2}}{t}$$

and

$$l_0 \equiv \overline{l(0)}$$

for $t = 10$, 20, 30, 40 and 50 sec.

The bar here implies an arithmetic mean over a group consisting of values of $l(0)$ in a range small compared with l_0. The values of $F(t, l_0)$ and l_0 are to some extent a function of the size of the group but it is felt that little falsification of the results arises on this account. Each group thus yielded five values of $F(t, l_0)$ and five corresponding values of l_0. It should be noted that $t = 0$ was not taken as the instant of release, so that, for instance, a series of observations lasting 70 sec provided 6 intervals of 10 sec, 5 of 20 sec, 4 of 30 sec and so on. The five values of l_0 were therefore not necessarily equal but in practice they differed so little that any one of them could be taken as representative of the group concerned.

4. Results

The results are tabulated in Table I. Values of l_0 range from 40 to 800 m ; values of $F(t, l_0)$ from $1{\cdot}7 \times 10^3$ to 129×10^3 cm^2 sec^{-1} and values of t from 10 to 50 sec.

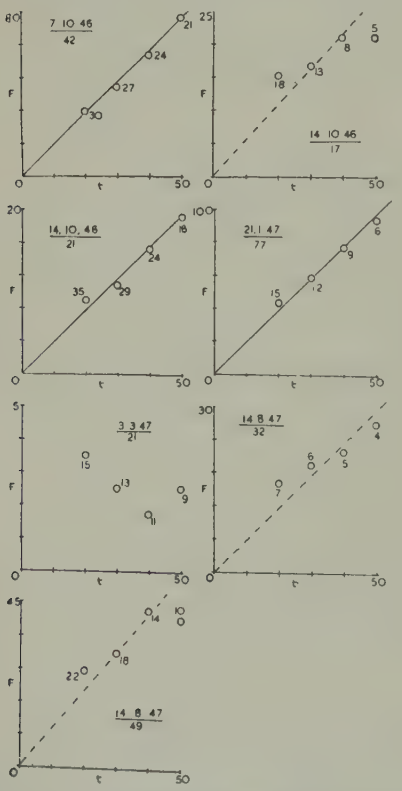

Figure 1. Observations made in light winds.

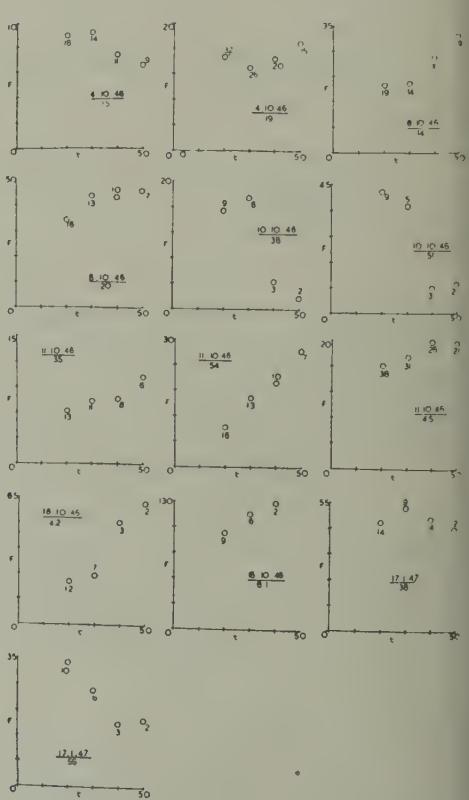

Figure 2. Observations made in stronger winds.

The ordinates are values of $F(t, l_0)$ in cm^2 sec^{-1} $\times 10^3$ and the abscissae, values of t in sec. The appropriate value of l_0 in cm $\times 10^3$ is shown beneath the date. The number by each point is that of observations on which it is based.

The effect of random errors of observation is to increase the value of $F(t, l_0)$, the relative increase being largest when the change of separation of the puffs is small, *i.e.*, in general for small separations and small time intervals. It became clear during the computation that the values mainly affected were those of $F(10, l_0)$ so that although they are tabulated they are not plotted in Fig. 1 which refers to observations made in winds less than 5 m sec^{-1} or in Fig. 2 which refers to observations made in stronger winds.

(a) *Results in light winds*

Of the seven sets of observations made in light winds, three show an approximately linear variation of $F(t, l_0)$ with t. Another three are roughly linear, the deviation being in the sense expected from errors of observation. The values computed from the observations made on 3 March 1947 show no systematic variation.

It seems reasonable to conclude that in light winds the time interval over which a linear relation between $F(t, l_0)$ and t must exist (independent of similarity theory) is in general greater than 50 sec. Previous writers such as Richardson (1926, 1929) Richardson and Stommel (1948) and Stommel (1950) appear to have disregarded the variation of $F(t, l_0)$ with t.

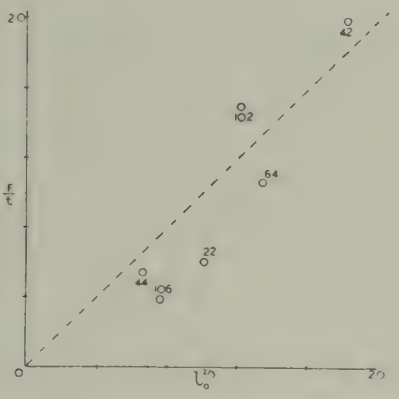

Figure 3. Observations made in light winds. The ordinates are values of $F(t, l_0)/t$ in cm^2 sec^{-2} $\times$ 10^2 and the abscissae values of $l_0^{2/3}$ in cm$^{2/3}$ $\times$ 10^2. The number by each point is that of the observations on which it is based.

Assuming this linear variation, and ignoring the results of 3 March 1947, the slopes of the lines which have been drawn in Fig. 1 so as roughly to fit the observations have been plotted against corresponding $l_0^{2/3}$ values in Fig. 3. No clear variation of $F(t, l_0)$ with l_0 is indicated – the scatter may be due to the variation of ϵ from one day of observation to another as well as to experimental error. A rough check of the prediction given in Eq. (1) can, however, be made by comparing values of ϵ computed from it with values estimated independently.

Values of ϵ computed from Eq. (1) on the assumption that $c_1 = 1$ lie within the range 0·2 to 0·9 cm^2 sec^{-3}. Brunt (1939) has estimated that the mean value of ϵ for the whole atmosphere is about 5 cm^2 sec^{-3} and that the mean value above the friction layer (and therefore relevant to this analysis) is about 2 cm^2 sec^{-3}. These values are if anything over-estimated and the computed values agree with them as well as can be expected.

A check on the self-consistency of the observations and analysis is obtained by calculating t_1, the maximum time interval for which the prediction in Eq. (1) is valid. Using $\alpha = 1$ and the computed values of ϵ and l_0, t_1 is found to be greater than 50 sec in every case ; the minimum value of t_1 is 800 sec. The observations and the value of ϵ computed from them are therefore mutually consistent.

(b) *Results in stronger winds*

Values computed from observations made in stronger winds are even more scattered. The values of $F(t, l_0)$ are of the same order as those found from observations made in light winds, but apart from two or three occasions when $F(t, l_0)$ appears to increase linearly with t no systematic time variation is shown. In general, however, $F(t, l_0)$ appears to increase with t.

Acknowledgments

I am grateful to Mr. C. S. Durst, who made available the original Orfordness observations. The note is published by permission of the Chief Scientist, Ministry of Supply, and of the Director of the Meteorological Office.

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551.521.3 : 551.576.1

The relation between thickness of cloud layers and their transmission of light

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(Manuscript received 22 January 1951, in revised form 8 May 1951)

SUMMARY

Apparent minimal cloud-transmission percentages for Bircham Newton are plotted against ertical distribution of clouds observed by aircraft, and cloud amount. With under 20 per cent nsmission great cloud thickness is possible, but with over 30 per cent no great thickness of low ud occurs. Percentage frequencies of minimal cloud transmission are given for a year.

1. INTRODUCTION

An attempt was made to get some information on the relation between the thickness cloud layers and their transmission of light, using a Burt vacuum sodium cell and a mbridge thread recorder at Bircham Newton, Norfolk, and at Aldergrove, N. Ireland. he apparent percentage transmission was determined by taking the minimum recorded lue for the period of an ascent by the aircraft Meteorological Flights at the same stations, a percentage of the value with clear sun and sky cloudless, or nearly so, at the same ur and upon the nearest day giving really satisfactory conditions. To be exact one should termine the illumination with the same solar altitude.

The percentage transmission has been termed ' apparent ' because the intensities are easured at ground level, whereas the incident light should be measured at the top of a ud at perhaps 20,000 ft. The radiation received is affected by scattering and by absorp- n due to smoke, also by the obliquity of the beam on the receiving surface.

Taking vertical incidence as traversing air mass one, with spectral energy k_λ, with creased air mass this is reduced to $k_\lambda f^{\sec z}$, where z is the solar zenith distance and f the ansmission factor. For incidence on a horizontal surface we then have $k_\lambda f^{\sec z} \cos z$. ow Abbot, Fowle and Aldrich (1922) found that for $\lambda = 0{\cdot}42\,\mu$, an extra-atmospheric tensity (at normal incidence) of 1·000 became 0·831 on the summit of Mt Whitney, ,000 ft; 0·764 on that of Mt Wilson, over 3,000 ft, and 0·586 at Washington. The figures late to cloudless but not necessarily hazeless days. Thus the extinction amounts to ·9 per cent while passing down to 15,000 ft and what remains loses 8·1 per cent tween 15,000 and 3,000 ft. The residue loses 23·3 per cent in reaching the level of ashington. These percentages are roughly in the ratio 2 : 1 : 3 and the corresponding .A.N. pressures may be taken as 0, 570, 900 and 1,000 mb respectively, so the last 100 mb s an effect approximately equal to all the rest of the atmosphere. Measurements of the tinction (namely scattering plus absorption) due to cloud and haze for known types and icknesses of cloud will accordingly be too low, because of the loss of light even on a ar day. There is a further complication. The cloud surface is entirely different from lane, since it is composed of spheres of diameters mainly 40-50μ (Houghton and Radford 38) for fogs ; 9-24μ for stratus and 9-15μ for cumulus (Mazur 1943). The difference tween reception on a plane surface and on a sphere was studied by Atkins and Poole 936, Figs. 5 and 6) and by Atkins, Ball and Poole (1937, Fig. 2). On a clear June day e total illumination I was 151 klx as measured using the globe photometer, whereas the rtical component V was 116 klx, both maximum values including reflexion from high uds. With solar altitude $\alpha = 63°$, $I = 137$ and $V = 113$ were obtained; but with $= 42°$, $I = 126$ and $V = 72$. Thus with low sun the droplets still receive sunlight at rmal incidence, but the vertical component received on a horizontal surface is much duced. Thus more truly comparable results would have been obtained had the globe otometer been used, but none was available for recording. The obliquity effect is in- cated in the graphs, for the apparent percentage transmissions are smaller at noon than r the 6-7 and 18-19 hr observations, all of which relate to 1943.

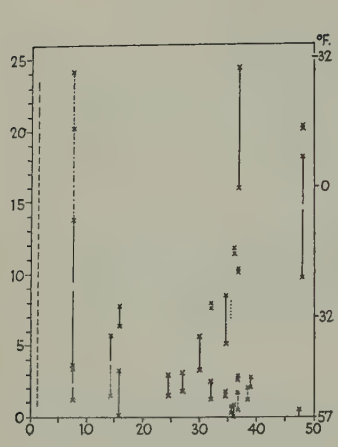

Figure 1.

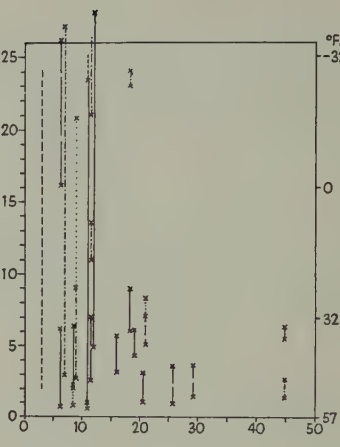

Figure 2.

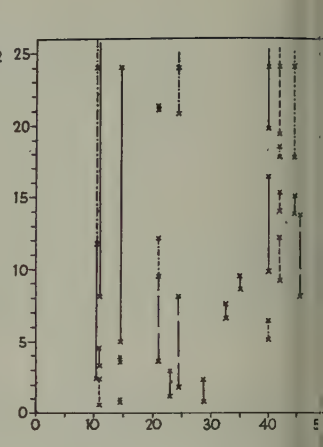

Figure 3.

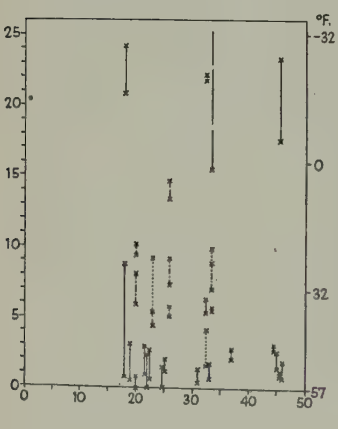

Figure 4.

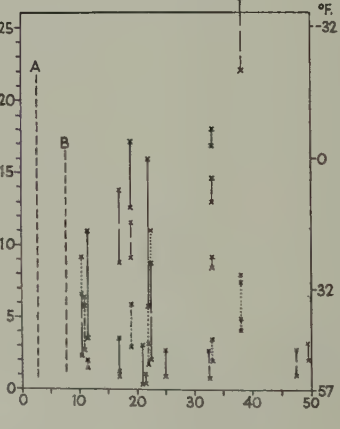

Figure 5.

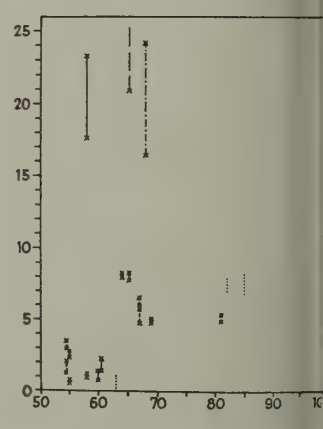

Figure 6.

Figures 1-6. In every figure left-hand ordinates show height in thousands of ft and right-hand the I.C.A.N. temperature scale. Apparent percentage transmissions are on the abscissae. A transmission measured without observations in the air, such as that of a thunderstorm, is shown by a line of dashes not limited by the crosses which normally mark the upper and lower boundaries of clouds. Cloud cover :

Full line = 10/10, one break = 9/10, two breaks = 8/10, dashes = 7/10, dash and dot = 6/10 — 4/10 inclusive, dots between limiting crosses = 3/10 or less – save for lofty cumuli or nimbostratus clouds the obscuration caused by these does not normally warrant their inclusion. Dots with no bounding crosses = a haze layer.

The figures relate to observations (GMT) at Bircham Newton in 1943 :
Figure 1. 06-07 hr, 17/5-30/6 ; Figure 2. 12-13 hr, 17/5-30/6 ; Figure 3. 18-19 hr, 17/5-30/6 ; Figure 4. 06-07 hr, and 18-19 hr, 1/7-24/8 ; Figure 5. 12-13 hr, 1/7-24/8. A, thunderstorm at 10 hr ; B, very heavy showers at 16 hr ; Figure 6. 06-07 hr and 18-19 hr, 17/5-24/8.

2. Observations at Bircham Newton

Bearing in mind the limitations inherent in the method adopted, the changes in ransmission associated with three types of weather were as follows. On 22 May morning pg gave a minimum transmission of 36 per cent at 5 hr for a thickness of 540 ft measured t 6 hr. On 25 May a depression moved from SW. England, E'N. to Felixstowe. The ircham Newton record showed 62 per cent transmission at 7 hr, which decreased irregu-rly to 16 per cent at 10 hr, and with temporary rises to 24 per cent, it showed a steady ll from 16 per cent at 13 hr to 6 per cent at 17 hr, rising to 11 per cent by 18 hr. On 6 June thundery showers reduced transmission from 80 to 12 per cent in a few minu'es.

Figs. 1-6 show the results for selected periods and hours of observation at Bircham Newton. The corresponding observations at Aldergrove have been omitted for brevity.

3. Discussion

Hewson (1943) has calculated the reflexion, absorption and transmission of solar adiation by fog and cloud for droplet diameters of 40μ (fog) and 10μ (cloud) for liquid-vater contents of 0·1, 1·0 and 5·0 g m^3 and for a series of thicknesses at 25°, 60° and 78·7° olar zenith distance. Fog 200 m thick and 1·0 g m^3 water content gives 37-38 per cent ransmission, irrespective of zenith distance. This agrees with the minimum found at ircham Newton on 22 May (Fig. 1) when the apparent transmission was 37 per cent, for 55 m thickness – the same value was given there another day for 244 m thickness. (The alculated values for 200 m of 0·1 and 5 g m^3 water content are 86 per cent and 7 per cent espectively.

On the other hand for cloud with 10μ diameter droplets, and 0·1g/m^3 6·2 per cent ransmission is given by 4,000 m thickness and 24 per cent by 1,000 m, the corresponding alues for 1·0 g/m^3 being 0·7 and 2·2 per cent. Actually 6 per cent transmission was the inimum found during the depression of 24 May, so the effect is nearer to 4,000 m thick-ess at 0·1 g/m^3.

Aldrich (1922) made observations of the reflexion of all wave lengths, using a pyrano-neter from a balloon above a level layer of cloud of indefinite extent. There was no change n reflexion within the range 33° to 69° solar zenith distance air mass (1·2 to 2·8). The eflexion was found to be 78 $\pm$ 1·1 per cent. According to Hewson this would require ver 1,000 m of 10μ cloud droplets with 0·1 g/m^3. With clouds of rather greater mass, s found by Mazur (1943), a smaller thickness would suffice. Again for Bircham Newton 200 ft of fog stratus beneath 1,400 ft of high stratus gave 16 per cent transmission. This s nearer to the expectation for a 10μ cloud of 0·1 g/m^3 than for a fog. Probably a range of rop sizes was present, the mean varying with altitude.

Fig. 1 especially shows that considerable thicknesses of unbroken high cloud may still low 35-40 per cent transmission. But 1,000-2,000 ft of low stratus cuts the illumination from 20-34 per cent around noon (Fig. 2), though a layer of stratus from 2,000 to 000 ft may transmit 30 per cent. The higher the base the greater the thickness of ie cloud layer for the same transmission, though stratus at six to nine thousand ft may e accompanied by only 18 per cent transmission leaving the suspicion that the lower mosphere may then have unusual pollution. The lofty cumulus or cumulo-nimbus, pecially with tops emergent above the total cover of the lower layers, may reduce the umination to 6 to 12 per cent if their bases are low, (*cf.* Figs. 1, 2, 3) but with bases over 000 ft, evening transmission may be over 25 per cent. Unrecorded thicknesses in thunder-orms have reduced transmission to 1·2 per cent. With these towering clouds much of ie reduction in transmission may be due to the light being diffused from the sides.

The records and flight observations obtained at Bircham Newton from June 1943 May 1944 were examined to obtain the percentage frequencies of apparent minimal ansmission for the maximum altitude of the sun as a function of cloud cover for low and ital cloud. The results for each month have been grouped for the seasons and for the ar with their cloud indications in Table I.

TABLE I. PERCENTAGE FREQUENCIES OF APPARENT MINIMAL TRANSMISSION PERCENTAGES GROUPED FOR EACH SEASON, WITH THE CLOUD INDICATIONS VALID FOR EACH TRANSMISSION

Transmission per cent	Percentage frequencies					Cloud indications
	Feb.-Apl.	May-July	Aug.-Oct.	Nov.-Jan.	Year	
0-10	0·0	3·3	4·4	6·8	3·6	Base low, great thickness possible
11-20	6·0	11·0	8·9	4·4	7·6	Great thickness possible
21-30	14·6	11·1	12·1	10·1	12·0	No great thickness of low cloud is
(0-30)	(20·6)	(25·4)	(25·4)	(21·2)	(23·2)	likely, much high cloud possible
31-40	10·9	14·2	31·2	5·5	15·4	No great thickness of low cloud
41-50	16·0	16·5	13·4	21·1	16·8	
51-60	9·7	13·1	8·9	13·6	11·3	Tenuous cloud
61-70	5·8	5·6	5·6	14·6	7·9	
71-80	17·4	5·5	5·5	5·7	8·6	Tenuous cloud or haze
81-90	5·0	3·3	3·3	4·4	4·0	
90-100	14·5	16·5	6·6	13·7	12·8	Clear, haze or very tenuous cloud

The cloud indications shown in Table I are well established, being based on the year's records and flight observations. But as a guide for aircraft as to cloud thickness and location the daylight records are not very helpful. No distinction can be made between the effects of superposed and single cloud layers. For example 1 Dec. was a clear day with a regular curve on the chart, little disturbed by clouds. This was taken as having 96 per cent transmission, that of 4 Dec., a clearer day, being accepted as 100 per cent. But on Dec. 2 and 3 the minima of transmission during the flights were exceptionally low, 6·5 per cent. On the 2nd the clouds were reported as 10/10 Ns, 870-660 mb, 7,230 ft thick, with 10/10 As, 600-360 mb, 12,190 ft thick. Below there was a haze top at 2,350 ft. The 3rd had 10/10 St with 4,200 ft cloud thickness, yet the recorded apparent transmissions were the same.

ACKNOWLEDGMENT

This work was undertaken while the author was temporarily in the Meteorological Office. He is indebted to the Officers of the Meteorological Flights at the two stations for the cloud observations and to the Meteorological Officers for care of the recorders. Thanks are due to the Director of the Meteorological Office for suggesting the work and for permission to publish it and to various colleagues for their assistance, especially to Miss D. Chapman for measuring the transmissions on the 1943-44 records from Bircham Newton.

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The heat economy of a rimed cylinder

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Summary

A method is described for calculating the surface temperature of a slender cylinder which is moved through a supercooled cloud and becomes covered with rime ice. When the surface temperature reaches 0 °C the ice formation proceeds at the maximum possible rate ; the associated liquid-water content of the cloud is calculated and found to be usually less than the maximum theoretical value inside a deep cumulus cloud. The rotating-cylinder technique of determining water content and drop-size distribution cannot therefore be used without consideration of the heat economy, and may otherwise lead to large underestimates of water concentration in severe icing conditions.

1. Introduction

In a previous paper (Ludlam 1950) the writer examined the heat economy of growing hailstones. When a hailstone falls through a cloud of supercooled droplets a substantial proportion of those in its path are caught and commence to freeze on its surface. The freezing is accompanied by the liberation of latent heat of fusion, and the surface temperature of the hailstone rises above that of the environment. The rate of freezing is controlled by the rate at which heat is transferred to the environment. The greatest rate of heat transfer occurs when the surface temperature of the hailstone reaches 0 C, and corresponds to a maximum rate of freezing. According to these calculations it frequently happens that supercooled water is caught at such a rate that it cannot all be frozen, so that liquid water accumulates on the hailstone until drops are shed into its wake. When the surface of the hailstone is wet, ice is deposited in a hard glassy form, but when impinging droplets freeze rapidly on contact, the ice is opaque and fragile, containing air enclosures.

Analogous icing phenomena occur when any object moves through a supercooled cloud. The riming of cylinders exposed from aircraft has been used to measure the liquid-water concentration in supercooled clouds, and even to provide some information about the drop-size distribution (see, *e.g.*, Dolozell, Cunningham and Katz, 1946). This is theoretically possible when several cylinders of different sizes are exposed simultaneously, because at a given wind speed the efficiency of catch of a cylinder depends upon its diameter and the size of the cloud droplets. It is therefore of interest to examine the heat economy of such cylinders and to determine the maximum rate at which cloud water can be frozen upon them.

2. The heat economy of a rimed cylinder

We consider a cylinder of diameter d, moving perpendicular to its principal axis at speed v through a cloud at a temperature $-T$ °C. In practice the rime deposit is kept symmetrical by rotating the cylinder ; we overlook the small increase in the heat transfer which may be expected on account of rotation at a speed small compared with v, and the small decrease due to the kinetic rise of temperature (of up to about 3 °C with an air speed of 200 mi/hr) which occurs in the air passing close to the cylinder, and we consider the rate of heat transfer from a unit area on the cylinder surface. As in the instance of a hailstone, the transfer of heat is accomplished predominantly by convection, and by the evaporation of ice (or water) from the surface ; the conduction of heat into the interior of the body and its transfer by radiation are small enough to be neglected.

For cylinders of diameter less than about 2 cm, mounted on aircraft moving at speeds

of up to 200 mi/hr, the Reynolds number, Re, is less than 10^5, and we may use the empirical equation (Fishenden and Saunders 1950)

$$H_1 = 0{\cdot}24(Re)^{0{\cdot}6}\, K\Delta T/d \quad . \quad . \quad . \quad . \quad (1)$$

where H_1 is the rate of loss of heat from unit area by convection, ΔT is the difference between the temperatures of the cylinder surface and the air, and K is the coefficient of conductivity of air. (If T is temperature (°C), $K = (573 + 1{\cdot}8T) \times 10^{-7}$ cal cm^{-1} sec^{-1} deg^{-1}. The value of K to be used in Eq. (1) is that appropriate to the temperature which is the mean of the temperature of the cylinder surface and that of the air).

A similar expression describes the rate of evaporation from the cylinder in terms of $\Delta\rho$, the difference between the vapour densities at the cylinder surface and in the environment, and k, the coefficient of diffusion of water vapour in air. (If p is the pressure in mb, $k = 3{\cdot}07 \times 10^{-3}\,(273 + T)^2/p$ cm^2/sec). Although the experimental data (*cf.* Frössling 1938, Karve 1942, Kramers 1946) concerning evaporation from wet spheres suggest that at these Reynolds numbers the numerical coefficient which modifies k may be somewhat less than that which modifies K, in the absence of an empirical coefficient for wet cylinders at the appropriate values of Re, and in view of the approximate equality of the thermal diffusivity of air and the molecular diffusivity of water vapour in air, we shall assume the evaporation to be increased by the same factor, which gives close agreement with the observations by Powell (1940) of evaporation from ventilated cylinders at Reynolds numbers of about 4×10^4.

We now introduce the latent heat of evaporation L to obtain a rate of heat transfer by evaporation H_2 :

$$H_2 = 0{\cdot}24(Re)^{0{\cdot}6}\, k\,\Delta\rho\, L/d \quad . \quad . \quad . \quad (2)$$

The total rate of heat loss $(H_1 + H_2)$ is thus given by

$$H_1 + H_2 = 0{\cdot}24(Re)^{0{\cdot}6}(K\Delta T + Lk\Delta\rho)/d \quad . \quad . \quad . \quad (3)$$

If m is the concentration of liquid water in the cloud, the rate W at which water arrives on unit area of the cylinder is

$$W = Emvd/\pi d = Emv/\pi \quad . \quad . \quad . \quad . \quad (4)$$

where E is the ' efficiency of catch ' of the cylinder. As each droplet reaches the ice surface it begins to freeze, and as the change of phase occurs the temperature of the supercooled liquid is raised to 0°C, so that a proportion of liberated latent heat is spent in this warming. After the freezing the ice cools to the steady-state temperature ($-T'$°C, say) of the cylinder surface. If the whole of the water which arrives at a temperature $-T$°C is frozen then the rate of heat transfer to the environment is

$$H_3 = Emv\,(80 - T + T'/2)/\pi \quad . \quad . \quad . \quad (5)$$

taking the latent heat of fusion to be 80 cal/g and the specific heat of ice to be $\frac{1}{2}$ cal/g. Since $H_1 + H_2 = H_3$ and $\Delta T = T - T'$ we may use Eqs. (3) and (5) to calculate the temperature of the iced cylinder.

In particular, when the cylinder-surface temperature has risen to 0°C and water is being frozen at the maximum possible rate, (ΔT and $\Delta\rho$, and therefore $(H_1 + H_2)$, of Eq. 3 taking their maximum values) we have (taking L as 600 cal/g)

$$Evm_c'\,(80 - T)/\pi = 0{\cdot}24(Re)^{0{\cdot}6}(KT + 600k\Delta\rho)/d \quad . \quad (6)$$

where m_c' is a critical value of the cloud-water concentration ; if this value is exceeded ice does not accumulate more rapidly upon the cylinder, but surplus liquid forms a skin upon it and is shed into the wake. m_c' is evidently the greatest cloud-water concentration which can be indicated by the accumulation of ice on the cylinder. The smallest cylinders in common use have diameters of about 0·35 cm, and we have computed m_c' as a function of air temperature when a cylinder of this breadth is used on an aircraft which flies at

90 m/sec (about 200 mi/hr) through two model cumulus clouds, whose base temperatures are 5° and 20°C. The value of m'_c is not particularly sensitive to small changes in air speed or cylinder diameter, being proportional to $(1/dv)^{0.4}$, and provided the bulk of the cloud water is held in droplets of at least several microns radius then variations in droplet size are even less important, E being practically 1·0. The variation of m_c' with air temperature, and the values (M) of m computed by assuming saturated adiabatic ascent of air from the bases of the clouds, are shown in Fig. 1.

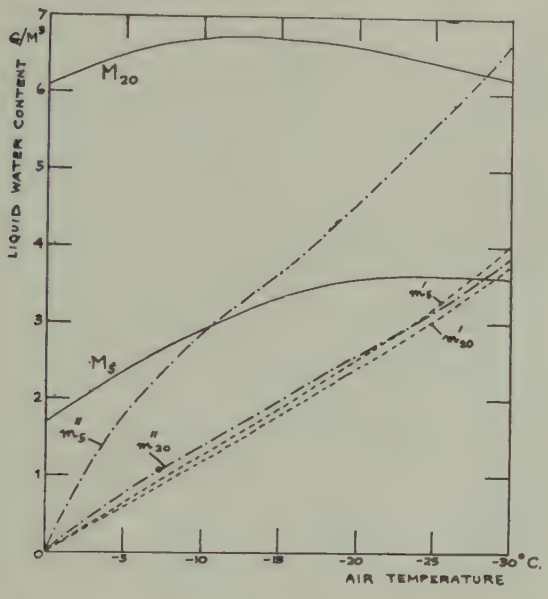

Figure 1. Theoretical saturated-adiabatic values M_s, M_{20} of liquid-water content in cumulus having base temperatures of 5° and 20°C at 900 mb, and the greatest values m_s', m_{20}' ; m_s'', m_{20}'' which could be indicated by the accumulation of rime ice on cylinders of diameter 0·35 and 15 cm respectively, moved through the clouds at 90 m/sec.

It is clear from the diagram that except at low temperatures in clouds with cold bases, rotating cylinders of this size are incapable of measuring the maximum liquid-water concentrations which may theoretically occur within cumulus clouds. Deductions concerning the amount of ice which may form on aircraft structures, and concerning water content and drop-size distribution in these clouds, cannot therefore be made without consideration of the heat transfers associated with the icing. In severe icing conditions ($m > m_c'$) we may expect the quantities of glaze forming on a set of slender cylinders to be proportional to $d^{-0.4}$, and any deduced values of cloud-droplet sizes would be quite fictitious. It would be interesting to know whether this conclusion is supported by any data so far obtained.

The calculations mentioned above have been repeated for a cylinder of diameter 15 cm, roughly corresponding to the leading edge of an aircraft wing, with results which are included in Fig. 1. The corresponding Reynolds number now reaches about 5×10^5, and the rate of heat transfer H_1 is described by the equation (Fishenden and Saunders 1950)

$$H_1 = 0{\cdot}024\,(Re)^{0{\cdot}805} K \Delta T/d \quad . \quad . \quad . \quad (7)$$

A corresponding adjustment was made to H_2, the rate of loss of heat by evaporation. The efficiency of catch E of the cylinder is considerably less than 1, and was calculated (from the formulae of Langmuir and Blodgett 1946) by assuming that the maximum liquid-

water content, M, was uniformly divided amongst droplets whose concentration amounted to 5×10^8/kg of air, *i.e.* several hundred per cm^3.

It will be seen from the diagram that in the cloud with a warm base and with E about 0·6, the critical water content hardly differs from the value indicated for the thin cylinder. In the cold cloud, however, the efficiency of catch E of the large cylinder is reduced to about 0·4 on account of the decrease in the assumed size of the cloud droplets, and the cylinder surface would not become wet at temperatures below about — 11°C.

3. Conclusion

It should be recognized that the maximum theoretical water contents of cumulus clouds must, if attained, be of small duration and extent. The characteristic widths of active clouds, which at any moment compose only a fraction of those present in the sky, are of the order of 1 km, a distance which is traversed by a high-speed aircraft in a few seconds. Larger clouds may be seen, but are composed of several active and a number of decaying individuals, so that average water contents deduced from measurements made over a minute or longer must necessarily fall below the maximum to be expected. Lewis and Hoecker (1949) have published measurements (made with riming techniques) obtained over periods of 10 sec which are significantly higher than previous values, reaching $1\frac{1}{2}$ to 2 g/m^3. These maxima were obtained at temperatures of — 10°C to — 15°C in clouds whose base temperatures appear to have varied between about 0° and 5°C. According to our diagram these values are about the greatest which their smallest cylinder of 1/8 in. (0·32 cm) diameter could be expected to indicate.

It must be concluded that published measurements of liquid-water concentrations obtained by rotating-cylinder techniques do not represent the maximum values which may occur in cumulus clouds. Moreover, they cannot in general be used to support theories which assume ' entrainment ' of dry air into a growing cumulus. There seems at present to be no good reason to suppose that the concentration of water in the form of supercooled droplets does not often reach several g/m^3 in large cumulus. Supercooled raindrops formed by the coalescence of cloud droplets (Ludlam 1951) may occasionally add to these values for brief periods during the development of showers.

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On the evaluation of terms of a type arising in Sutcliffe's treatment of cyclonic development

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Summary

A method is described for the approximate evaluation of expressions of the form $\mathbf{V}.\nabla\zeta$ in which $\mathbf{V}$ denotes a geostrophic wind vector, and ζ a geostrophic vorticity. The method is suitable for application to synoptic charts.

1. Introduction

In applying dynamical ideas to synoptic charts it is often necessary to evaluate expressions of the form $\mathbf{V}.\nabla\zeta$, where $\mathbf{V}$ denotes a geostrophic wind and ζ a geostrophic vorticity. For example, Sutcliffe (1947) introduces the quantity $-V'\,\partial/\partial s\,(\zeta_{500}+\zeta_{1000}+l)$ as a measure of cyclonic development; here V' denotes the thermal wind taken through a substantial layer of the troposphere, in practice 1,000 to 500 mb, $\partial/\partial s$ denotes differentiation in the direction of the thermal wind, ζ_{500} and ζ_{1000} denote the vorticities of the geostrophic wind field at 500 mb and 1,000 mb respectively and l is the Coriolis parameter.

If charts of vorticity, ζ, are drawn, expressions such as $\mathbf{V}.\nabla\zeta$ can be evaluated by measurements of the magnitude of the geostrophic wind vector, and of the appropriate gradient of vorticity, but the method is lengthy and laborious. The method now to be described enables an individual evaluation to be made in less than two minutes. Besides providing a convenient tool for research, the method gives hope that theories involving such expressions may be applied quantitatively on a routine basis in synoptic analysis and forecasting.

2. Theoretical basis of evaluation

On a synoptic chart, the geostrophic wind and its vorticity are usually derived from contours of an appropriate isobaric surface. The thermal wind is similarly derived from a set of thickness lines.

Suppose then that a geostrophic wind field is derived from a set of isopleths of h. Taking horizontal rectangular axes of x, y with the axis of x directed along the geostrophic wind direction at the origin o, the geostrophic speed V at o is given by

$$V = -\frac{g}{l}\frac{\partial h}{\partial y} \qquad . \quad . \quad . \quad . \quad . \quad (1)$$

where g is the acceleration due to gravity.

Suppose also that a field of geostrophic vorticity is derived from a set of isopleths of H. The vertical component of geostrophic vorticity ζ is given by

$$\zeta = \frac{\partial}{\partial x}\left(\frac{g}{l}\frac{\partial H}{\partial x}\right) - \frac{\partial}{\partial y}\left(-\frac{g}{l}\frac{\partial H}{\partial y}\right)$$

$$= \frac{g}{l}\left\{\frac{\partial^2 H}{\partial x^2} + \frac{\partial^2 H}{\partial y^2}\right\} - \frac{v_E \cot\phi}{R} \qquad . \quad . \quad . \quad (2)$$

where v_E denotes the eastward component of the geostrophic wind field defined by the isopleths of H, R the radius of the earth, and ϕ the latitude. Except near the equator where the technique is not applicable, the last term in Eq. (2) is negligible.

Then from Eq. (1), and Eq. (2) with the last term omitted, at the origin

$$\mathbf{V}.\nabla\zeta = -\frac{g}{l}\frac{\partial h}{\partial y}\cdot\frac{\partial}{\partial x}\left\{\frac{g}{l}\left(\frac{\partial^2 H}{\partial x^2}+\frac{\partial^2 H}{\partial y^2}\right)\right\}$$

$$= -\frac{g^2}{l^2}\frac{\partial h}{\partial y}\left\{\frac{\partial^3 H}{\partial x^3}+\frac{\partial^3 H}{\partial x\partial y^2}\right\} \quad . \quad . \quad . \quad (3)$$

the terms arising from differentiation of l being neglected.

To evaluate, for example, $\mathbf{V}_{1000}.\nabla\zeta_{1000}$, where $\mathbf{V}_{1000}$ denotes the geostrophic wind vector at 1,000 mb, we have only to identify both h and H in Eq. (3) with h_{1000}, the height of the 1,000 mb surface, choosing the axis of x in the direction of $\mathbf{V}_{1000}$ at the origin.

In what follows, we shall for simplicity consider the evaluation of $V'\,\partial\zeta/\partial s$, which is of the type arising in Sutcliffe's treatment of development. Choosing the axis of x in the direction of the thickness line at the origin, and replacing h in (3) by h', the value of the thickness,

$$V'\frac{\partial\zeta}{\partial s} = -\frac{g^2}{l^2}\frac{\partial h'}{\partial y}\left\{\frac{\partial^3 H}{\partial x^3}+\frac{\partial^3 H}{\partial x\partial y^2}\right\} \quad . \quad . \quad . \quad (4)$$

at the origin. The terms omitted from Eq. (4) involving variations of the Coriolis parameter are usually much smaller than the terms retained, and do not materially affect the features of the computed field of development.

The expression within the brackets on the right of Eq. (4) may be approximated by means of finite differences (Southwell 1946). For this purpose we consider the value of H at the network of points illustrated in Fig. 1; these form part of a square grid with side of length a.

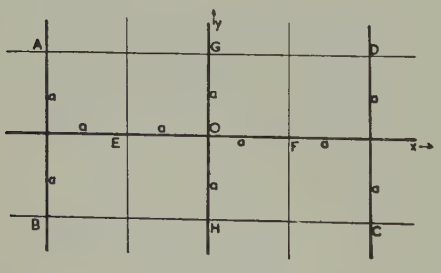

Figure 1.

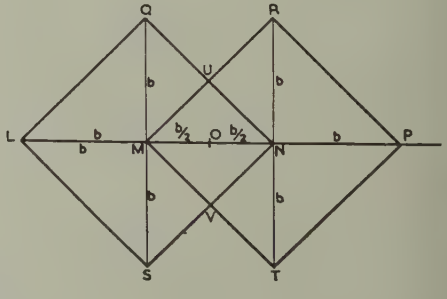

Figure 2.

We may express the values of H at the points A, B, C, D, E and F in terms of the value of H at the central point O and its derivatives by means of a Taylor series. The grid length a, will be regarded as a small quantity and the choice of an appropriate length in practice will be discussed in a later section.

Thus we have

$$H_A = H_0 - 2a\left(\frac{\partial H}{\partial x}\right)_0 + a\left(\frac{\partial H}{\partial y}\right)_0 + \frac{1}{2}\left\{4a^2\left(\frac{\partial^2 H}{\partial x^2}\right)_0 - 4a^2\left(\frac{\partial^2 H}{\partial x\partial y}\right) + a^2\left(\frac{\partial^2 H}{\partial y^2}\right)_0\right\}$$

$$+\frac{1}{6}\left\{-8a^3\left(\frac{\partial^3 H}{\partial x^3}\right)_0 + 12a^3\left(\frac{\partial^3 H}{\partial x^2\partial y}\right)_0 - 6a^3\left(\frac{\partial^3 H}{\partial x\partial y^2}\right)_0 + a^3\left(\frac{\partial^3 H}{\partial y^3}\right)_0\right\}$$

$$+ \text{terms in } a^4$$

and $$H_E = H_0 - a\left(\frac{\partial H}{\partial x}\right)_0 + \frac{a^2}{2}\left(\frac{\partial^2 H}{\partial x^2}\right)_0 - \frac{a^3}{6}\left(\frac{\partial^3 H}{\partial x^3}\right)_0 + \text{terms in } a^4$$

together with similar expressions for H_B, H_C, H_D and H_F with appropriate modification of the signs.

It follows directly from the addition (and subtraction) of these equations that

$$H_C + H_D + 4H_E - H_A - H_B - 4H_F = 4a^3 \left\{ \left(\frac{\partial^3 H}{\partial x^3}\right)_0 + \left(\frac{\partial^3 H}{\partial x \partial y^2}\right)_0 \right\} + \text{terms in } a^4 \quad . \quad . \quad . \quad . \quad (5)$$

Further, $$h'_H - h'_G = -2a \cdot \frac{\partial h'}{\partial y} + \text{terms in } a^2 \quad . \quad . \quad . \quad . \quad (6)$$

Thus multiplying together Eqs. (5) and (6), neglecting terms in a above the fourth power and then substituting in Eq. (4)

$$\left(V' \frac{\partial \zeta}{\partial s}\right)_0 = \frac{g^2}{8l^2 a^4} \left\{ H_C + H_D + 4H_E - H_A - H_B - 4H_F \right\} \left(h'_H - h'_G \right) \quad . \quad (7)$$

Thus Eq. (7) provides a finite difference approximation for the expression which it is required to evaluate.

An alternative and somewhat more compact network of points for carrying out the evaluation is formed by two over-lapping squares (Fig. 2) of which the diagonals are of length $2b$.

The finite difference approximation on this network is

$$\left(V' \frac{\partial \zeta}{\partial s}\right)_0 = \frac{g^2}{l^2 b^4} \left\{ H_P + H_R + H_T + 5H_M - H_L - H_Q - H_S - 5H_N \right\} \left\{ h_V' - h_U' \right\} \quad . \quad (8)$$

3. Choice of grid length

The most suitable size of the grid length (a or b in Eqs. (7) and (8) above) for use in practice is determined by two main considerations. First, if the grid length is chosen too small, the errors and irregularities in the basic chart will have undue prominence and may even be greater than the differences which it is proposed to estimate. On the other hand the dimensions of the scale must not be too large compared with the size of the synoptic system which it is proposed to investigate. A small depression has a diameter of about 1,000 miles but somewhat smaller synoptic systems are also important.

It is convenient to choose units and grid length so that the multiplying factor in Eq. (7) namely $g^2/8l^2 a^4$ has a convenient integral value. If H and h' are measured in units of 100 ft, as is customary in synoptic analysis, and the hour is used as unit of time, then $g^2/8l^2 a^4 = 10^{-2}$ for latitude 50° when $a \simeq 100$ n. mi. This has been found to be a convenient grid length in practice for synoptic purposes. The corresponding length for b in Eq. (8) is 168 n. mi.

For practical use a grid of the form indicated by the thicker lines in Fig. 1 is engraved on a sheet of transparent material and placed on the synoptic chart with EF parallel to the isopleths of h' at o ; values are then interpolated at A, B, C, D, E, F, G and H from the appropriate set of isopleths of h' or H which have been drawn carefully at 100 ft (or 200 ft) intervals. These values are inserted in Eq. (7). If the scale is made to a grid length of 100 n. mi the expression $\{ H_C + H_D + 4H_E - H_A - H_B - 4H_F \} (h_H' - h_G')$ gives the value of $V' \partial\zeta/\partial s$ directly in units of 10^{-2} hr^{-2}.

The Coriolis parameter l varies with latitude ; also the scale of a synoptic chart is usually variable from one latitude to another. Consequently if a scale of the dimensions indicated above is used over a synoptic chart of an extensive area a correction will have to be applied at latitudes other than 50°. The correction factors appropriate to a conical orthomorphic chart with standard parallels 30° and 60° are given in Table I (easily-remembered approximate values are in brackets).

TABLE I.

Latitude	30°	35°	40°	45°	50°	55°	60°	65°	70°
Factor	2·62(2·5)	1·88(2)	1·46(1·5)	1·17(1)	1·00	0·92(1)	0·94(1)	1·04(1)	1·08(1)

If, for speed of routine working, uniformity of unit over the whole chart is abandoned, and the correction factors are not applied, it must be remembered that values about latitude 35° are twice as effective for development as the same values near latitude 50°.

4. Application of the scale to evaluation of Sutcliffe's development expression

Sutcliffe's expression for the relative divergence D through the 1,000-500 mb layer may be written (Sutcliffe 1947)

$$D = -V' \frac{\partial}{\partial s} (\zeta_{500} + \zeta_{1000} + l) \quad . \quad . \quad . \quad (9)$$

Although variations in Coriolis parameter play a major role in 'long-wave theory' synoptic experience shows that they are small compared with variations of ζ_{500} and ζ_{1000} in regions of development. In any case they may be handled separately when this technique is applied. D can therefore be evaluated by applying the scale to isopleths of the 1,000-500 mb thickness together with (a) contours of the 1,000 mb surface and (b) contours of the 500 mb surface. This involves two separate operations, the results of which must be added. In practice it is quicker to add graphically the heights of the 1,000 mb and 500 mb surfaces, h_{1000} and h_{500}, to form new isopleths of $h_{1000} + h_{500}$. From a chart with these isopleths superimposed on the thickness lines (isopleths of $h_{500} - h_{1000}$), the evaluation of D can be carried out in a single operation by means of the transparent scale. Values to be inserted in Eq. (7) are then $h' = h_{500} - h_{1000}$ and $H = h_{500} + h_{1000}$ interpolated from the two sets of isopleths at appropriate points. From a series of such evaluations isopleths of the relative divergence can be constructed.

5. Example

Figs. 3(a), (b) and (c) present an example of a chart of Sutcliffe's development term computed as indicated in the preceding paragraph.

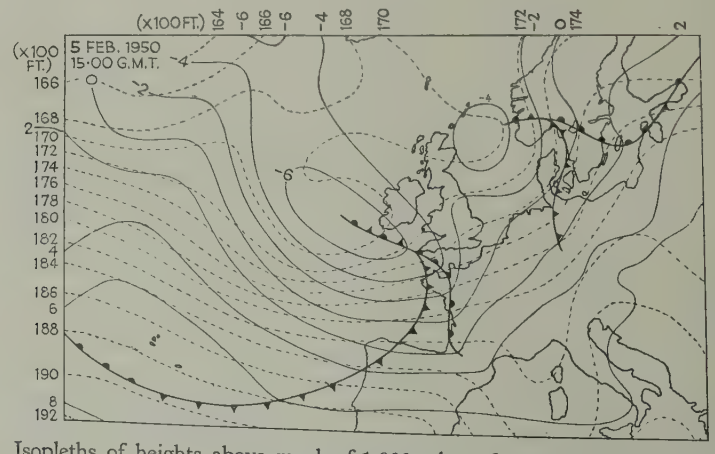

Figure 3(a). Isopleths of heights above m.s.l. of 1,000 mb surface —— and 500 mb surface - - - (intervals of 200 ft)

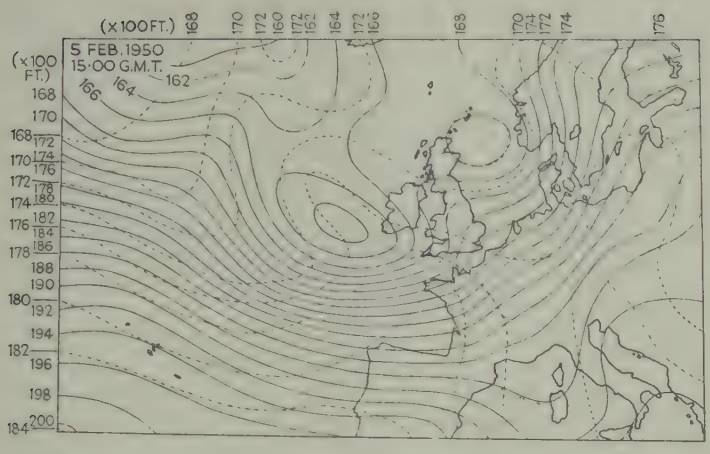

Figure 3(b). Isopleths of the sum —— and difference - - - of the height of the 500 mb and 1,000 mb surfaces (intervals of 200 ft)

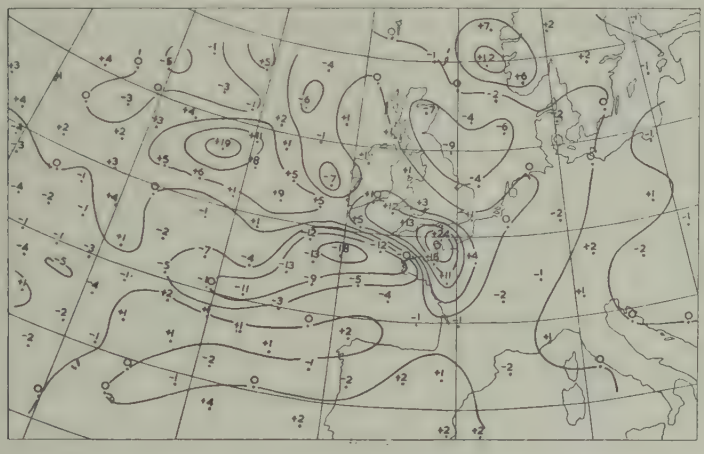

Figure 3(c). 5 Feb. 1950, 1500 GMT. Isopleths of the Sutcliffe development term (units 10^{-2} hr^{-2} + cyclonic, — anticyclonic).

Acknowledgment

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551.506.1

The demise of the annual Greenwich Meteorological Report

By Sir HAROLD SPENCER JONES, F.R.S.,
Astronomer Royal

The ' Reports of Observatories ' has been a feature of the *Quarterly Journal* since its first appearance in 1871. From that date the Royal Observatory, Greenwich, has contributed annually to the series, which thus contains a review of the meteorological activities conducted at Greenwich over a period of nearly 80 years.

The Magnetic and Meteorological Department of the Royal Observatory was established by George Biddell Airy, Astronomer Royal, in 1840, the first Superintendent being the distinguished meteorologist James Glaisher. Since then a continuous series of meteorological observations has been maintained for over 109 years covering the inclusive period 1841-1949. The initial purpose of the department was to furnish data that had a bearing upon the astronomical work of the Observatory, but at a time when meteorological stations were few and far between, the Royal Observatory, with its long tradition and proximity to London, quickly assumed a position of importance in meteorology which few observatories today could hope to attain. With the growth of the science and the great expansion in the number of observing centres the Observatory might have become, so far as meteorology is concerned, merely one of an extensive network of meteorological stations. But year by year the increasing length of its records became of growing importance for climatology, so that it continued to enjoy a position of distinction and came to be regarded, rightly or wrongly, by a large body of the English public as the centre of information regarding the weather. The idea is still wide-spread in the public mind that the Astronomer Royal is responsible for the weather.

Today the value of the Greenwich meteorological observations lies without question in their continuity and in their homogeneity. In the first report submitted to the *Quarterly Journal* in 1871 we find reference to the Robinson and Osler anemometers which have served continuously to furnish respectively the Greenwich data concerning air movement and wind direction and pressure, the former since 1866 and the latter since 1841. In later years there was a justifiable reluctance to change to equipment and methods which had become standardized elsewhere, because observations with new equipment would not have been directly comparable with the earlier observations.

The early reports appear to have been devoted mainly to accounts of the progress of work during the year and included statements upon the installation, modification and adjustment of equipment, the progress of publication of results and brief accounts of any special investigations undertaken during the period under review. It is interesting, for instance, to read in Vol. 3 of the *Journal*, ' Previous to the year 1875, the graduations of our thermometers rested fundamentally upon those of a standard thermometer (the property of Mr. Glaisher), which derived its authority from comparison with original thermometers constructed by the late Rev. R. Sheepshanks in the course of his work of restoring the National Standard of length.' Following this the standard was one prepared at Kew and supplied to the Royal Observatory.

In 1893 the second Superintendent, Ellis, retired and was succeeded by W. C. Nash and in the report for 1894 there appeared the first specific comments upon temperatures, rainfall and sunshine recorded during the year. Such comments continued to appear in subsequent reports and with the accession of W. W. Bryant to the post of Superintendent in 1903 the comparison of observations secured during the year with those recorded in the earlier Greenwich volumes became the principal feature of the report, a feature which has persisted to the present day.

The Royal Greenwich Observatory is in the process of transfer to Herstmonceux. The continuation of meteorological observations at Greenwich on the same scale as hitherto

s now no longer possible. A skeleton series of meteorological observations is being main-ained for the time being on the old site at Greenwich, principal amongst which are sunshine nd night sky records secured for the purpose of comparison with similar records being aken at Herstmonceux. Ultimately, a full-scale programme, similar to that formerly naintained at Greenwich, but using standard equipment, will be undertaken at Herst-nonceux. The annual Greenwich Meteorological Report consequently disappears from he *Quarterly Journal*. To those readers of the *Journal* who have found these Reports, ummarising the principal features of interest in the year's weather at Greenwich, a source f information and enjoyment, the discontinuance of the long series of meteorological bservations at Greenwich will be regarded with regret.

Some remarks on the annual trend of weather in the Scottish Highlands

By H. Flohn

In a very noteworthy paper H. H. Lamb (1950) has revealed the structural elements of the annual trend of weather in the British Isles. The present note based on a statistical evaluation of the excellent Ben Nevis observational series is a further contribution to the subject. Beside the work of C. E. P. Brooks, Belasco and others, as mentioned already by Lamb, Ellis's (1896) paper on cloudiness at Greenwich and Mossman's (1898) paper on the older London non-instrumental observations must also be noted.

Fig. 1 shows the mean trend of temperature on Ben Nevis computed from diurnal means (Omond 1910) and of lapse rate between Fort William and Ben Nevis (mean pressure, 857 mb), thus practically giving the lapse rate of the ground layer (Schneider-Carius 1947) between 850 and 1,000 mb. These two curves are almost completely inverse and thereby show the overwhelmingly advective nature of annual variation of temperature, with unstable cold air and stable warm air. Buchan's cold spells from Feb. 7 to 14, April 11 to 14, May 9 to 14, and especially June 29 to July 4, are confirmed by these 20-year mean values ; the two cold spells in August and November are shifted by a few days. Of particular interest is the characteristic cold wave from 25 February to 3 March which is accompanied by a very pronounced trough of low pressure. The connection with the simultaneous occurrence of high pressure over Greenland and the subsequent frequency peak of cyclonic activity in the Mediterranean areas, as pointed out by Lamb (*loc. cit.*), is a well-known feature to the experienced European meteorologist.

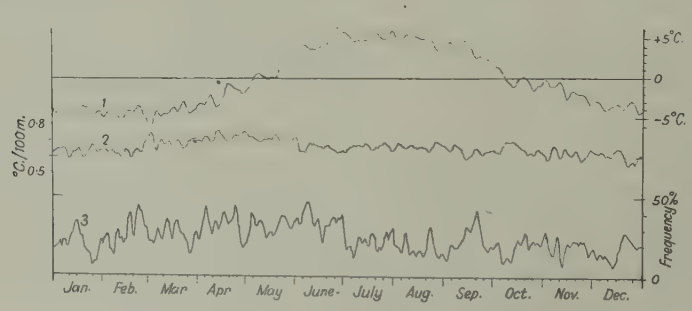

Figure 1. Ben Nevis (Oct. 1884 to Sept. 1903)
Curve 1 .. daily mean temperature
2 .. mean lapse rate (°C/100 m)
3 .. mean frequency of days without precipitation (3-day means).

The high frequency of precipitation on Ben Nevis (275 days per ann.) permits the use of the frequency of days without precipitation (Fig. 1) as a simple index of the occurrence of anticyclonic types. This index undergoes considerable variations in the course of the year. If we neglect the weak annual variation, the following short-period variations give frequency values (overlapping 3-day means) which are above the significance level of three times the standard variation σ :

Maximum frequency of days without precipitation (anticyclonic) : February 24, April 6 to 8, 25, June 7 to 11, September 22.

Minimum frequency of days without precipitation (cyclonic) : January 26, November 13, December 13 to 14.

While the peaks of cyclonic frequency (probability of precipitation over 95 per cent) are in perfect agreement with the dates given by Lamb, this is true to a lesser extent of the anticyclonic frequency peaks, though the latter all appear in Brooks's (1946) frequency curves of anticyclonic days.

The physical origin of the singularities in the annual trend of weather and their possible relation to the general circulation of the atmosphere, especially to the quasi-periodic oscillations of the stratospheric polar vortex in winter, as examined recently by the author (1950), must await more extended discussion.

The author is very indebted to the President of the German Zonal Meteorological Organization, Prof. Dr. Weickmann, for permission to publish this note, and wishes to express his gratitude to Mr. H. H. Lamb for his valuable help.

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German Weather Service,
U.S. Zone,
Bad Kissingen.
5 May 1951

551.511 : 551.524 : 551.558.1

Vertical convective heat flux

By R. B. Montgomery

One aim common to my paper on 'Vertical eddy flux of heat in the atmosphere' (Montgomery 1948) and to Dr. Robinson's essay on p. 61 of this volume is a precise and suitable definition of 'vertical eddy flux of heat' or 'vertical convective heat flux.' It is interesting to see how the two definitions agree. My definition is that at any point the vertical (upward) eddy heat flux per unit area is $\rho \overline{W'h}$, where ρ and h are instantaneous values of density and specific enthalpy respectively, the bar indicating the mean value of the variable enclosed. If the motion is such that the mean vertical mass flux is zero, W' is simply the instantaneous upward component of velocity, and $\rho \overline{W'} = 0$. In general, W' is the instantaneous upward component of velocity relative to a steadily-moving local level surface through which the mean mass flux is zero, so again $\rho \overline{W'} = 0$.

Robinson specifies a velocity component w_1 with reference to an ambiguous 'level,' and I shall guess that by w_1 he means the quantity represented here by W' (and in my paper by V_z'). He assumes that the turbulent fluctuation of pressure is negligible. He defines the heat transported by an element as the 'heat which would be made available if the element, having been arrested in the level, were brought at constant pressure to the mean state of the air at this level.' Although he does not say so, this imaginary process is impossible if the element differs in composition from the mean air, so he excludes fluctuation of composition. (Or does he imagine the element brought to the mean *temperature* of the level?) Therefore the vertical eddy heat flux per unit area is

$$\overline{\rho W' (h - \bar{h})} = \overline{\rho W' h}$$

Hence, for the special case to which it is restricted – no fluctuation in either pressure or composition – his definition is consistent with my unrestricted definition. The simplicity of Robinson's method is possible owing to the restrictions. My definition is justified by a thermodynamic study of a specific open system. The two ways of approaching the definition supplement each other nicely.

It is useful to collect the following practically equivalent formulae for the unit-area eddy heat flux in air of uniform composition :

$$\overline{\rho W'h} = c_p \overline{\rho W'T} = c_p \overline{\rho W'T'} = c_p \bar{\rho}\, \overline{W'T'}.$$

For actual air of variable composition the last three also represent, with perhaps negligible error (c_p now being an average specific heat), the loosely defined ' eddy flux of sensible heat ' per unit area. Because the proportional fluctuation of density is of the same order of magnitude as that of absolute temperature, there is no justification for writing $c_p \bar{\rho}\, \overline{W'T}$, which is equivalent to a formula occurring at one point in the classical paper by Taylor (1915, top of p. 3) and again in the book by Lettau (1939). Robinson gives another formula, $(Mc_p/\mathbf{R})p\overline{W'}$, where $\mathbf{R}$ is the universal gas constant and M is the mean molar mass. This formula follows from the equation of state for a perfect gas, $p = (\mathbf{R}/M)\rho T$, if the fluctuations of pressure and composition are negligible. The effects of these two fluctuations are discussed in turn.

The proposition that the proportional turbulent fluctuation of pressure is small compared with those of density and absolute temperature appears above twice in Robinson's formulation of heat flux. Although he exaggerates the evidence by saying that the proportional fluctuation of pressure is of the order of 10^{-3} times that of absolute temperature (his source, Calder 1949, p. 74, gives the fluctuation of temperature as 0·1°C to 1°C, the proportional fluctuation of absolute temperature therefore being 10^{-3}, and gives the proportional fluctuation of pressure as $\frac{1}{2}(\bar{\rho}/\bar{p})u'^2 \simeq 2 \times 10^{-5}$ for a velocity fluctuation of 2 m/s, corresponding to a mean wind some one to ten times this speed), the proposition seems fully justified for light winds. For sufficiently strong winds, however, the proportional fluctuation of pressure can be expected to approach and even exceed that of absolute temperature. It is desirable that for a variety of natural conditions this important proposition be tested with direct simultaneous observations. Robinson calls the proposition ' apparently justifiable ' without indicating any restrictions, but such universal validity seems unlikely.

The fluctuation of composition is not generally negligible. As

$$\frac{1}{M} = \frac{1-q}{M_d} + \frac{q}{M_w},$$

where q is specific humidity, the equation of state leads to the following relation between the proportional changes of pressure, density, and absolute temperature and the change of specific humidity :

$$\frac{dp}{p} = \frac{(M_d/M_w - 1)dq}{1 + (M_d/M_w - 1)q} + \frac{d\rho}{\rho} + \frac{dT}{T}$$

Specific humidity in the atmosphere is a small number, so $(M_d/M_w - 1)q$ in the denominator is negligible. The ratio of the humidity term to the temperature term is therefore

$$\left(\frac{M_d}{M_w} - 1\right) T\frac{dq}{dT} = 0{\cdot}6087\, T\frac{dq}{dT}.$$

Above the ocean the ratio of the fluctuations of specific humidity and temperature is approximately equal to the ratio of the departures from values at the interface, so dq/dT frequently exceeds 5×10^{-3} °C^{-1} (see, for instance, Montgomery 1940, Tables I, II). The ratio of the humidity term to the temperature term therefore frequently exceeds unity. It follows that Robinson's unit-area heat-flux expression $(Mc_p/\mathbf{R})p\overline{W'}$, resulting from neglect of the pressure and humidity terms in comparison with the density and temperature terms, is not valid over most of the earth.

I wish to contest Robinson's Sections 3 and 4, where he criticizes two parts of my paper.

In order to express the eddy heat flux as the product of an eddy diffusivity and a suitable gradient, I defined a function $Y = h + \phi$, the sum of specific enthalpy and geopotential, and then (as $\rho W'\overline{Y} = \overline{\rho W'h}$) defined the eddy diffusivity K by

$$\overline{\rho W'h} = -K\rho\, \partial\overline{Y}/\partial z$$

The function Y was chosen because, unlike h, it is nearly conservative for simple vertical displacement of a parcel. The change in a displacement dp is, where α' and α are specific volumes of parcel and surroundings, only $dY = (\alpha' - \alpha)dp$. Robinson (Section 3) states the fact that dY is the work done by unit mass of parcel against the buoyant force (gravity plus pressure-gradient force per unit mass). By obscure deduction he concludes : ' In this respect therefore Montgomery's treatment shows no advance on the Taylor-Brunt method – he has replaced the somewhat unsatisfactory ' mixing length ' deduction of the relation between convective heat flux and potential-temperature gradient by a straightforward assumption of proportionality but has been obliged to neglect the buoyancy forces in applying this method to heat transfer.' Actually the equation above is a straightforward *definition* and involves no neglect or assumption. I cannot see that the small deviation from conservatism is of any significance in this connection.

This method of defining an eddy coefficient of viscosity or diffusivity goes back to Boussinesq, Ekman, and Richardson. Like a molecular transport coefficient, the eddy coefficient is defined as the ratio of a unit-area flux to a gradient. The flux and the gradient are clearly defined and, at least in principle, measurable, so the coefficient is calculable. I believe Robinson has confused this method with Taylor's method, in which the eddy coefficient is defined as $\overline{W'l}$, the eddy being assumed to have originated at the level $z-l$. The coefficient $\overline{W'l}$, although not definitely defined or generally measurable or necessarily equal to the ratio of unit-area flux to gradient, has the advantage of possessing an intuitive relation to the mixing process. Whereas $\overline{W'l}$ is inherently positive, the coefficient I have used includes the effect of both thermal and mechanical turbulence and, as shown by Ertel and by Priestley and Swinbank, may on occasion be negative. Obviously a coefficient that can become negative is of limited usefulness, but, on the other hand, if the coefficient is to be used quantitatively there are clear advantages in its being fully defined and calculable.

In Section 4, Robinson says there appears to be fallacy in my assertion that, in general, the eddy heat flux at a point depends on an arbitrary quantity (T_0) and cannot be specified absolutely in terms of the local phenomena (W', T, q, *etc.*) alone. May I first point out that what he presents as a direct and complete quotation from my paper is altered in minor respects and, in the middle, lacks two entire sentences ? The reader of Robinson's section is deprived of the essential consideration, again *variable composition*. The explanation of my assertion is furnished in one of the missing sentences : ' This sort of dependence is inherent in a problem dealing with unsteady flow in a medium of variable composition, because there is an arbitrary constant in the enthalpy scale of each component.' In objection, Robinson says that ' the total heat (enthalpy) per unit mass is a single-valued function of state,' but his statement should be restricted to a given closed system or to a material of fixed composition. Section 4 contains further statements that appear fallacious, but I agree that ' the point is of some importance.'

The point is this : suppose that through a surface there is an exchange of equal masses of two substances, for example, dry air and aqueous vapour ; what is the heat transfer ? For simplicity, the two substances can be at the same temperature and pressure. As a result of the exchange the side receiving aqueous vapour (greater specific heat) would increase its capacity to emit heat on isobaric cooling to a given lower temperature, so the heat transfer could be said to coincide in direction with the vapour transfer and to equal the extra heat that could be emitted in the imaginary cooling. Such a heat transfer is specified in terms of an arbitrary process that is independent of the local phenomena ; for given local phenomena the heat transfer in the direction of transfer of a given one of the substances can have various values, both positive and negative, depending on the process. The question is incomplete except in the special case that the surface is isobaric and the two substances

are of the same composition, the only case considered by Robinson. In the general question the word ' heat ' is perhaps used in a sense beyond its strict meaning.

The difficulty is due to fluctuating composition, not to change of phase, but the small fluctuation in composition of air is of importance because water changes between solid, liquid, and vapour phases. The side receiving aqueous vapour gains the possibility of realizing a higher temperature as the result of condensation, so the ' heat latent in the vapour ' can, to practical advantage, be included in the heat transfer. Some arbitrary choice remains : the latent heat for the temperature at which the water vapourized (ocean surface), or for the temperature at which the vapour will condense (cloud), or for some other temperature ? One arbitrary choice is suggested in my paper, and a related problem has been encountered by Jacobs (1949).

Another approach would be to say that the upward eddy flux of vapour is equalled locally by an imaginary precipitation of liquid or solid water of chosen temperature, so that the difference in properties of the two aqueous streams provides a definite flux of enthalpy.

Variable composition offers problems even in the formulation of the unit-area ' eddy flux of sensible heat,' which I have called ' loosely defined ' because its meaning is elusive. Certainly this quantity is not represented by $\overline{\rho W' c_p T}$ with c_p variable, but I have assumed that $c_p \overline{\rho W' T}$ is an approximate representation.

I wish to acknowledge gratefully Dr. G. K. Batchelor's helpful criticism. The U.S. Office of Naval Research has sponsored this research under contract with Brown University.

Brown University,
Providence, R.I.
21 April 1951

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Reply

By G. D. Robinson

1. An apology

I would like at the outset to apologise to Professor Montgomery for the annoyance which my inadequate quotation of his paper must have caused. I shall refer to his original paper as M. 1948 and to mine as R. 1951.

2. A definition of heat transfer

Some, if not all, of our differences are due to the different meanings which we attach to the word *heat*. In this reply, (as it was usually, but not invariably, in R. 1951) heat transfer will be defined qualitatively as energy transfer resulting from a difference of temperature, and quantitatively in the following way. Starting with the concept of temperature a thermal insulator is defined as a material across which a given difference of temperature, once established, is maintained indefinitely without outside interference. A system sur-

bounded by a thermal insulator is then considered. The work w done in changing such a system from one specified state to another is independent of the source of work and of the intervening states of the system – this is a statement of the first law. We say the internal energy has changed and write

$$w = \Delta u$$

If now the system is enclosed by walls which are not thermally insulating and is brought from the one specified state to the other by doing work w' on it we say the system has absorbed an amount of heat q where

$$q = \Delta u - w'$$

This defines q. This definition is attributed by Guggenheim (1949) to Born. The important property of heat transfer so defined is that it implies a difference of temperature. There may or may not be a transfer of energy between two systems exchanging matter at the same temperature, but there is no transfer of heat.

I chose my definition of the eddy flux of heat to have a reasonably satisfying intuitive basis, and at the same time to depend on a *closed* system (the element) and a process (cooling at constant pressure to the temperature of the surroundings), to which the first law could be properly applied, and for which q could therefore be evaluated. The heat flux defined in this way is what I believe meteorologists to mean by ' flux of sensible heat ' In a medium of fluctuating composition I see no objection to the use of my definition, with the substitution of ' temperature ' for ' state.'

Montgomery's definition of the eddy flux of heat implies a quite different meaning of the word, heat flux being equated to flux of thermal plus potential energy (where I use the term potential energy to include geopotential ϕ and the energy pv which is a consequence of the pressure of the surroundings). If ' heat ' is to have this meaning I do not think its convective transfer presents any more difficulty than does convective transfer of matter.

3. Heat transfer in a medium of fluctuating composition

' Heat flux ' through a surface of zero mass flux, as defined by Montgomery, contains an unknown term. If there is no fluctuation of composition this term can be eliminated ; if composition is variable I agree that the unknown term cannot be eliminated. I now see that my criticism on this point was based on a partial misunderstanding of M. 1948. But I cannot agree that this unknown term depends in any way on the history and future of the elements of air responsible for the energy flux. I maintain that this flux is expressible in terms of

(a) Local phenomena – the observed temperatures and pressures, and the concentrations of each component.

(b) Known properties of the components, *e.g.*, the specific heats at all temperatures.

(c) Unknown (but constant) constants – the total heat of each component at an arbitrary reference temperature fixed once for all, most logically the absolute zero.

I agree of course that prior and subsequent changes of temperature in the systems on either side of the surface through which transfer takes place cannot be specified solely in terms of phenomena in the surface, but these temperature changes depend only in part on the heat flux as defined by either Montgomery or myself.

Montgomery now poses the simple example of exchange of 1 g of air for 1 g of water vapour at the same temperature and pressure, and asks ' what is the heat flux? ' My answer is ' zero ' on my definition, and ' the difference in the specific total heats of air and water vapour at the specified temperature and pressure ' on Montgomery's definition. This difference is unknown but not arbitrary in the sense of being an accident of history or of the particular set of tables to hand at the moment. We have indeed a choice of tables, but having made our choice we must stick to it at all times and places. Montgomery's answer to his question, unless I misinterpret him completely, is ' it depends on what happens to the air and water vapour during an unspecified time before and after the moment of exchange.' With this I cannot agree.

I accept Montgomery's criticism so far as it concerns the influence of fluctuating composition on density in the application of my approximate formula $c_p\overline{pw_1}/R$. I was, however, thinking in terms of convection over land, and the few – probably inadequate – measurements of temperature and humidity fluctuations available to me suggested a relative effect of humidity not more than 1/10 of that in Montgomery's marine examples.

4. Heat transfer in a medium subject to pressure fluctuations

The need for the restriction to negligible pressure fluctuation is clear in my definition. I believe it to be implied in Montgomery's, but apparently he does not accept this contention. We are both concerned with a surface through which there is no net transfer of mass, and use this condition to define the surface. In the notation of M. 1948 v_z is the vertical velocity of an element, $V_z = \overline{\rho v_z}/\overline{\rho}$, and $V_z' = v_z - V_z$, so that $\overline{\rho V_z'} = 0$. Montgomery's V_z' corresponds to my w_1 ; his V_z is the 'upward speed of a steadily moving surface through which there is no net mass transfer.' Now in the derivation of his expression for the heat flux the following expression occurs (M. 1948, p. 267).

$$\Delta t\iint v_z p\,dS = \Delta t\iint V_z p\,dS + \Delta t\iint V_z' p\,dS$$

'p' being identical in each term. Montgomery identifies the first term on the right with the work of expansion of the open system whose upper (open) boundary is defined by V_z. I have difficulty in appreciating arguments depending on the work done by movement of an open boundary specified by mean values (I am not alone in this, see *e.g.*, Miller (1950)), but can only assume that this identification implies that p is a time mean of pressure, since V_z is a weighted time mean of velocity. But in the term on the left the identical p is identified with the pressure of its surroundings on the element, and it is through the second term on the right that the pv term in the total heat of the element enters the expression for the energy flux. I can only reconcile these apparent contradictions if the pressure is always the same.

I entirely agree that evidence concerning the significance of pressure fluctuations in the atmosphere is inconclusive. The estimates I have seen are not measurements but 'Reynolds pressures,' deduced from measured velocity fluctuations. These beg the question of the validity of the Reynolds formulation consequent on the difficulty of specifying a mean velocity in atmospheric motion. I hoped, however, that my remarks would emphasize the fact that some commonly accepted formulae are valid only if the pressure fluctuations, which imply local deviations from hydrostatic equilibrium, are negligible.

5. The eddy diffusivity for heat

I can reply more briefly to Professor Montgomery's complaints against my section 3. There is, of course, neither assumption nor neglect involved in taking the quotient of measures of any two entities, but the act of investing this quotient with more than a trivial arithmetical significance may involve both. In Montgomery's definition of the eddy diffusivity for heat this act implies that the function Y 'must be unchanged by simple transportation of air to a different level' (M. 1948, Section 2). This implies neglect of the work done by the buoyancy forces, *i.e.*, of the buoyancy forces themselves. It is in this sense only that I referred to Montgomery's definition as showing no advance on the Taylor-Brunt method.

Kew Observatory,
Richmond, Surrey.
28 May 1951

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551.513.1

A hemispherical study of the atmospheric angular-momentum balance

By W. van der Bijl

Though Starr and White (1951) discuss the more important of the many questions as to he validity of their statistical procedures used, I am disappointed that their critical remarks pp. 223-4) do not mention anything about the daily persistence in the wind observations.

It is certain that daily observations of the wind at the same station (lag 24 hr) do show nutual dependence. But I wonder whether this persistence will disappear in the space-veraging which Starr and White have performed. This is of importance, as the application f the law of propagation of errors (p. 219, Eq. 7) requires the mutual independence of he elements of the sample.

If it could be shown that the daily values of $[v]$, $[u][v]$ and $[uv]$ are persistent, then ne generalized law of propagation of errors can be used (see, *e.g.*, Chapman and Bartels 1940).

The large number of observations for each level (about 180) permits the computation f the autocorrelation coefficients, which are necessary to derive the true standard errors.

I am surprised to see that such a special paper as that of Starr and White has to deal vith no fewer than 3 of the 5 statistical effects, discussed in my paper (1951). These nree are : (1) the sampling effect (Starr and White, p. 219), (2) the selection effect (p. 223, emark (d) in their paper) and (3) the persistence effect.

De Bilt,
Con. Ned. Met. Inst.
4 July 1951

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Reply

By V. P. Starr and R. M. White

The time series referred to are almost devoid of serial correlation. Thus, for example, ie actual values of these correlations for a lag of one day for $[v]$, $[u]$ $[v]$, $[uv]$ and $[u'v']$ e, in order, + 0·122, + 0·070, + 0·103 and + 0·129 at the 10,000 ft level during the ix-month period used. At the 30,000 ft level the corresponding values are — 0·037, + 0·067, + 0·062 and + 0·039. This situation is to be expected, since the individual aily space averages are formed by using reports from a sparse network of stations. Owing the effect of missing reports the selection of stations represented varies from day to day o that the serial correlation present for individual stations is largely suppressed in the pace averages. It therefore appears that the simple procedure actually applied is sufficient or the quality of the data used. However if it were possible to make a similar study in-olving the use of extremely accurate measures of the individual daily space averages, there a good probability that a more pronounced serial correlation would exist in the data, in hich case it would become necessary to make use of more elaborate statistical procedures suggested.

We both wish to thank Mr. Van der Bijl for his interest in our work. It is in the rrect treatment of data such as are under discussion that answers lie to many fundamental oblems of meteorology.

Massachusetts Institute of Technology,
Cambridge, Massachusetts.
9 August 1951

551.578.11
551.576.1
551.574.1

Cloud Physics

A discussion on Cloud Physics was held on 20 June 1951, the President, Sir Robert Watson-Watt, C.B., F.R.S., being in the chair.

The opening speaker was A. C. Best (Meteorological Office, London), who read his paper ' The size of cloud droplets ' (*Q.J.*, **77**, p. 241). He was followed by F. H. Ludlam (Imperial College, London) on ' The production of showers by the coalescence of cloud droplets ' (*Q.J.*, **77**, p. 402). Dr. R. S. Scorer (Imperial College, London) then read his paper on ' Billow clouds ' (*Q.J.*, **77**, p. 235). The paper by E. J. Smith (C.S.I.R.O., Australia) on ' Observations of rain from non-freezing clouds ' (*Q.J.*, **77**, p. 33) was read in title only.

Mr. B. J. Mason : In his introduction Mr. Best states that cloud droplets have diameters within the range 5 to 40μ and that his paper is an attempt to explain why this should be so. Although Mr. Best has made a rough estimate of the time for which a droplet remains in a layer cloud he has not *explained* why cloud droplets attain the observed size in that time because he has *assumed* a value for the prevailing supersaturation. This procedure largely begs the question. The existing degree of supersaturation is controlled mainly by the size and concentration of the droplets and the rate of cooling of the air ; it is not a fixed quantity during the cloud-forming process but changes as the droplets grow. In other words, the supersaturation and the growth rate of the droplets are interdependent quantities, one should not consider them as independent or take one as fixed, as Mr. Best has done. By assuming a fixed value for the supersaturation one ' straight-jackets ' the growth of the droplets and the physics of the problem, *i.e.*, how the supersaturation is achieved is thereby largely ignored. One is then left with the rather trivial problem of substituting in the well-known but rather dubious equations governing droplet growth. To decide what values of the supersaturation to adopt Mr. Best appeals to the observed values of droplet size ; but as his paper sets out to explain how these sizes arise, this procedure virtually assumes the answer at the outset.

I feel that as the supersaturation is as important as the lifetime of the droplet in the cloud in determining its final size, any satisfactory attempt to explain the constitution of clouds must involve simultaneous computation of the supersaturation and droplet size, and a discussion of the factors which will govern the supersaturation. A first step in this direction has been taken by Howell (1949). Although his results indicate that condensation processes in uniformly cooled air are not in themselves sufficient to explain the observed inhomogeneity in cloud-droplet spectra, I think his work represents the biggest advance so far – particularly as Langmuir's ' time of rise theory ' is irrelevant to droplet growth in natural clouds. Furthermore, Howell has dealt with a fairly realistic population of droplets and not with just a single droplet.

Mr. E. Gold : Does the formula of Professor Sutton for σ apply to cloud ? I should have expected σ in the free air to depend rather on the wind shear than on the absolute velocity of the wind.

Mr. A. C. Best (*in reply*) : The purpose of this paper – as indicated in the summary – was to show that the mean life of an individual layer-type cloud particle, as governed by turbulent diffusion, is consistent with the mean size attained by cloud particles. The mean size is controlled by the supersaturation. The value assumed for the supersaturation was based upon the published measurements of nucleus mass. I am in complete agreement with Mr. Mason's remarks about the physics of cloud-particle formation but find it difficult to see their relevance to this particular paper.

With regard to Mr. Gold's question, Sutton obtained the value 1·75 for m (in Eq. 7) from examination of the scatter of small free balloons after travelling distances up to 600 km (Richardson and Proctor, *Mem. R. Met. Soc.*, 1, No. 1, 1925). The value of c in the same equation was based upon the expansion of shell bursts at heights of the order of 1,000 m.

Mr. I. C. Browne : In his absence, Mr. L. G. Smith has asked me to make a contribution for him. In a famous paper on rain electricity (*Geophys. Mem.*, No. 84), Simpson records some instances where heavy rain fell at Kew from clouds which were giving only very small electric fields. Since many theories suppose that ice is essential to the formation of rain electricity, it seemed worth finding out whether the clouds which gave the uncharged rain extended above the freezing level. The Meteorological Office kindly supplied the estimated cloud tops for the days in question - it should be noted that these heights are not observed, but estimated from a tephigram. The accompanying table summarises the relevant data. It will be seen that in all cases the clouds are shallow compared with the average thundercloud, while in several cases the clouds appear to be non-freezing. Since all the instances recorded here relate only to Kew, it would seem that heavy non-freezing rain is by no means an uncommon occurrence in England and that the Bergeron hypothesis is inadequate not only for tropical but also for temperate climates.

Date	Time (GMT)	Rainfall rate (mm/hr)	Height of freezing level (ft)	Height of cloud top (ft)
24. 9.43	2346	12	6,200	4-6,000
9. 6.44	1223	29	8,600	10,000
15. 7.44	1758	42	10,300	7,700
2. 9.44	1406	26	6,300	12,000
26. 9.44	0349	32	9,300 4,600	6,500
26.10.44	0803	17	10,000	6,000
29. 6.45	1929	42	10,200	8,600
13. 9.45	1931	32	7,000 9,800	9,500

Mr. L. C. W. Bonacina : In case they should bear on the condensation processes under discussion, I should like to point out two kinds of cumulus clouds - those that grow upwards and those that grow downwards. Ordinarily a cumulus develops into cumulonimbus by towering slowly upwards and fraying out in false cirrus at its edges. But occasionally the head of a cumulus appears before the body which condenses out with great rapidity suggesting a high degree of supersaturation.

In this respect I well remember the sudden onset of the great London thunderstorm of 16 June 1917 when 4·5 in. of rain was measured at Campden Hill, Kensington. In a hot steamy atmosphere at 1 p.m. a few orange-coloured cumulus tops appeared which speedily joined up as the blue sky deepened, at first into dark curtains and then into solid phalanxes of black cumulonimbus from which rain was falling and thunder rattling with only a dangerously brief warning.

Mr. E. Gold : The work of Köhler on the sizes of drops in cloud has not I think had the recognition it deserves. Ten years or more before Findeisen's work, Köhler found the drops in clouds were of sizes about 7-8μ or multiple of these sizes by 2, 4, 8, *etc.* I pointed out in a review of Köhler's work that if there were a cloud with drops originally of uniform size and these began to coalesce, then the separation under gravity would tend to produce drops of 2, 4, 8, *etc.*, times the original drop size and so eventually cause rain by coalescence. Has Mr. Ludlam considered the ' layering ' of the cloud by this gravitational separation ?

In making these comments I do not wish to detract from the interest of the papers of Mr. Best and Mr. Ludlam nor from the seriousness in the one case and the gaiety in the other, of their presentations.

Mr. E. R. Woodcock : The conclusions reached by Mr. Ludlam depend to some extent on the value he assumed for the efficiency of collection E. He generalizes Langmuir's early calculations by a method similar to that used by Das (*Indian J. Met. Geophys.*, **1**, No. 2, p. 137, 1950) and finds that a value of unity is a fair average value to take for E.

However all these results are based on the assumption of potential flow which is not necessarily true and on other simplifying assumptions. Both Langmuir and Das agree that their calculated values of E are likely to be overestimated. Langmuir has performed a few experiments with glass spheres falling through viscous liquids and concluded that the value of E should be about 0·2. On the basis of these results therefore it would seem that there is considerable doubt about the value of E and that perhaps $\frac{1}{4}$ may well be a better estimate than unity. Has Mr. Ludlam taken these considerations into account ?

Dr. F. J. Scrase (*communicated*) : Towards the end of his paper, Mr. Ludlam concludes that raindrop breaking does not occur in clouds with bases having temperatures below about 0°C and that thunder is not produced by these clouds. He suggests that raindrop breaking may always play an active part in the separation of thunderstorm charges. I made a similar suggestion in 1938 to explain the observed differences in the electrification of rain from shower clouds and thunderstorms, when I pointed out that the lower region of positive charge which is characteristic of the active discharge centre of a thunderstorm is absent, or at any rate less well developed, in a shower cloud without thunder. (*Geophys. Mem.* IX, No. 75, 1938). I suggested that violent thunderstorms develop initially in the same way as shower clouds but that the strong vertical air currents with which they are associated cause the development, by the breaking-drop process in the lower parts of the clouds of relatively small regions of positive charge, which produce the observed preponderance of positively charged heavy rain and provide the strong local fields that are necessary to cause electrical discharges.

I find also that Mr. Ludlam's suggestion that clouds with cold bases do not produce thunder is supported by evidence from electrical soundings in clouds made at Kew in 1936-1937 (Simpson and Scrase, *Proc. Roy. Soc.*, A, **161**, p. 309, 1937). Of eighteen storms and shower clouds in which electrical fields were recorded on the alti-electrograph, ten were thunderstorms. In these storms the cloud bases had temperatures averaging about 10°C and were about 1·7 km below the freezing level. The corresponding figures for the shower clouds were 4°C and 0·6 km. These differences are in accordance with Mr. Ludlam's evidence.

Experimental evidence justifying Mr. Ludlam's assumption that every collision is followed by coalescence has recently been provided by Gunn and Hitschfeld (*J. Met.*, **8**, p. 7, 1951) whose laboratory experiments show that this is the case for the sizes of drops and droplets that they used.

Mr. A. J. Whiten (*communicated*) : Concerning these papers on cloud physics, it has occurred to me that nearly all research in this field seems to have been confined to instability cloud. My remarks have been somewhat forestalled by those of Mr. Mason who spoke of work on Aldergrove aircraft ascents, but I wonder whether this field has been recognized as being of even more significance than that of convective cloud ? To my uninformed mind, it is not so easy to imagine an ice-topped nimbostratus cloud at low level as a fully developed cumulonimbus with ice top. The physics of this type of rain cloud appears to me to be even more obscure than that of the convection cloud. I have in mind, of course, the typical warm-front cloud which gives drizzle at first, increasing to heavy ' soft ' rain about the passage of the front. When this cloud first becomes effective along a given line, it seems to me that it must be relatively thin by comparison with its later development, so that on any assumption based on growth by collision, the drops falling therefrom would be of small diameter, and having a much greater distance to fall than later, would tend to evaporate before reaching the surface ; whereas the drops falling from the base of the much thicker cloud near the front would be much larger and with a shorter distance to fall would in consequence reach the ground in larger form than from the front part of the cloud. This is, of course, what actually happens, but it seems to me that the change from drizzle to heavy rain might be expected to be a quicker business than it is in fact, and that a more or less sudden onset of rain might be a much more common warm-front phenomenon than it is.

It would be interesting to know how far the computations involved in the physics of convection clouds can be taken to apply to stratiform clouds.

Mr. F. H. Ludlam (*in reply*) : Mr. Smith's figures, given by Mr. I. C. Browne, are nteresting and emphasize the need for a more critical examination of our own rain clouds. t will be generally agreed, however, that a deduction of cloud tops from soundings made t some distance is usually very uncertain.

As Mr. Gold says, Köhler was the pioneer in the study of cloud droplets, and if he s no longer quoted it is only because his work has become part of the now generally accepted oundations on which other researches rest. Some of his ideas, however, are not proof gainst criticism, and in particular I do not agree that one should expect to find only roplets of geometrically progressing radii in an originally homogeneous cloud.

In reply to Mr. Woodcock, the values of E derived by Langmuir are interpolations etween values appropriate to high Reynolds numbers (potential flow) and low Reynolds umbers (viscous flow). Das has made calculations assuming potential flow but that the roplets obey Stokes's Law, and finds for droplets of comparab'e size values which excee d oth those indicated by Langmuir's formulae and those I have used. However, I do not egard the assumptions of Das as any better than those I have made in modifying Langmuir's E. When droplets of comparable size approach each other the flow near them is likely to e distorted from any simple theoretical solution, and experimental observations may be xpected to provide more readily acceptable answers. The necessary careful experiments ave not yet been made, but it may be mentioned that after examining the drop-size dis- ribution during the ageing of an artificial cloud Findeisen (1932, quoted above) concluded hat coalescence of droplets of $r < 10\mu$ proceeds with about the maximum possible value f E (which approaches 4 with droplets of nearly equal radius). I believe the estimates of E which I have used to be the best which can at present be made, and that they are likely o be underestimates rather than overestimates.

Dr. Scrase tends to confirm that thunderstorms do not occur in clouds with cold ases. It would be interesting to know whether thunderstorms often occur in the tropics ith clouds wholly below the 0°C level (*cf.* H. Foster, *Bull. Amer. Met. Soc.*, **31**, p. 140, 950).

The present calculations are certainly not applicable to the production of rain from ayer clouds, in which both the updraught and water content are much smaller and more ifficult to estimate. I agree with Mr. Whiten that they deserve further study. The gradual ncrease of frontal rain may be due to a slow increase in the thickness of the cloud-wedge ver a period of some hours.

Mr. E. Gold : I understood that Helmholtz waves occurred where there was a dis- ontinuity of density as well as of wind. Would not Dr. Scorer's contention also prove hat there could be no waves in the sea ?

Dr. R. C. Sutcliffe : It would be interesting to know whether visual observations an throw any light on the cloud types which Dr. Scorer is discussing. Do we know whether he cloud features are physical entities moving with the wind or of the lenticular type with he air flowing through them ? My own impression is that both types occur but I have ot related my casual observations with other factors. Perhaps there are members present ho could give a more useful opinion.

The technique of cinema photography should be able to resolve much of the con- roversy on the dynamical structure of the various forms of cellular, roll-like or wave-like louds.

Mr. A. J. Whiten : At about 1130 hr on 9 June 1950, I observed a number of rolls f cirrocumulus, of which only the ends were visible. Through the gap of clear sky an ircraft flew, leaving wisps of condensation trail only in the spaces between the strips of loud. While I make no attempt to explain this phenomenon, it may be inspiring to those ho know more about the subject than I do.

Dr. R. S. Scorer (*in reply*) : To Mr. Gold I would reply that the waves on the sea re mainly stable waves that are travelling relative to the water, and their velocity is nothing o do with the wind speed. The process of their generation is extremely complicated, and certainly not anything like an unstable (Helmholtz) perturbation. There is no doubt, owever, that the energy of sea waves comes from the motion of the atmosphere, while

there is no similar source of energy for atmospheric waves ; many of the theoretically possible wave motions for the atmosphere therefore simply never occur. In particular there is scarcely ever anything in the atmosphere analogous to sea waves, the only notable case being tidal waves.

In answer to Dr. Sutcliffe, I would say that there is neither theoretical reason nor visual evidence to suggest that billow clouds move at a speed significantly different from the air that surrounds them or of a layer very close to them. Nephoscope observations on the speed of billow clouds are quite valid. Some lenticular wavy clouds do move relative to the air but they are stationary relative to the ground, unless the appearance of the sky is changing rapidly, and so nephoscope observations on them would not be attempted.

Though cinema photography may reveal many fascinating cloud formations and transformations I cannot agree with Dr. Sutcliffe that it will resolve controversies on their dynamical structure. Some forms have been described mathematically, one or two explained, but of most cloud forms no mathematical description has yet been offered. In my paper I simply offer a mathematical description of a possible motion that I think often occurs in the air and which, I think, explains most billow clouds better than descriptions previously offered. Cinema photography can only reveal, at most, the motion in the air containing cloud, and this may not be where the controversy lies. It is more likely to reveal a kind of motion that it is beyond our power to describe mathematically.

We are likely to be faced with more problems than to have existing ones resolved as we watch the sky more closely. Mr. Whiten's remarks illustrate this well. The fascinating occupation of sky-watching should not be discouraged even if it does create headaches for the mathematicians.

Proceedings at meetings of the Society

Ordinary Meeting, 20 June 1951

The following candidates were declared to have been elected that day by the Council :

Fellows

NDERSON, ARCHIBALD T. R., 47 Stanley Grove, Penwortham, Preston, Lancs.
ICKENSON, ALISTAIR FREDERICK, 2038 Metcalfe Street, Montreal, P.Q., Canada.
ALLACHER, JOHN G., 2 Northfield Crescent, Longridge, West Lothian, N.B.
ENSTOCK, HERBERT, Ph.D., M.Sc., " Glengariff," Caerwys, North Wales.
RESS, Flt. Lt., LESLIE GEORGE, Officers' Mess, R.A.F. Flying College, Manby, Nr. Louth, Lincs.
ULLAN, Lieut. Com., EDWIN RONALD, R.D., R.N.R., 26 Clement Street, Darwen, Lancs.
WARD, ALAN, 1 Mediterranean Terrace, Gibraltar.

Associates

ALL, FRED KEITH, 12a Lucas Street, Caulfield S.E.8, Victoria, Australia.
OVENEY, CHRISTOPHER RAYMOND RALPH, 28 Clarey Avenue, Apt. 1, Ottawa, Ontario, Canada.
USHING, FRANK LOUIS, A.F.O., R.C.A.F. Station, Gimli, Man., Canada.
UTZMAN, WALDEMAR LEONARD, D.A.F.O., Tower Building, Montreal Airport, Dorval, P.Q., Canada.
ROUP, ALEXANDER JAMES, 25 Elizabeth Street, Mentone S. 11, Victoria, Australia.
WHEELER, CHARLES GARNET, R.C.A.F. Station, Greenwood, N.S., Canada.

Foreign Members

ERSON, A. FELICJAN ANDRZEY, Dr. Phil., Näckrosvägen 9, Solna, Stockholm.
GROODIN, IVAN S., 12 Lawes Street, Hawthorn, Victoria, Australia.

The following papers were read and discussed :

(a) *The size of cloud droplets* by A. C. Best (*Q.J.*, **77**, p. 241).

(b) *The production of showers by the coalescence of cloud droplets* by F. H. Ludlam (*Q.J.*, **77**, p. 402).

(c) *Billow clouds* by R. S. Scorer (*Q.J.*, **77**, p. 235).

(d) *Observations of rain from non-freezing clouds* by E. J. Smith (*Q.J.*, **77**, p. 33) was read in title only.

Summer Meeting, 18 July 1951

The Summer Meeting of the Society was held on 18 July 1951 at the East Malling Research Station, East Malling, Maidstone, by courtesy of the Director of the Station. Some 45 members and their friends were welcomed at 2.45 p.m. by the Director, Dr. F. R. Tubbs. The party spent a most enjoyable and interesting afternoon in being shown over the extensive grounds and learning something of the many activities of the Station, including an indication of the operation of meteorological factors in fruit research. A full account of the meeting was published in the August number of *Weather*.

Reviews

The climate near the ground. By Rudolf Geiger. A translation by Milroy N. Stewart and others of the 2nd (1941) German Edition of *Das Kilma der Bodennahen Luftschicht* with revisions and enlargements by the author. Cambridge, Massachussetts (Harvard Univ. Press), 1950. 8vo. Pp. xxi, 482 ; 181 figures. $5.00.

Das Klima der Bodennahen Luftschicht (*Ein Lehrbuch der Mikroklimatologie*). By Rudolf Geiger. Braunschweig (F. Vieweg), 1950. 3rd, revised and enlarged edition. 8vo. Pp. vii, 460 ; 195 figures. No price given.

From the albedo of a butterfly's wing to the phenomenally low temperatures of the *Gstettneralm* ' frost bowl ' Professor Geiger's book leaves untouched practically no microclimatic feature of practical consequence and human interest. These two editions and especially the appearance of one of them in English translation will be welcomed by climatologists and users of the science alike. They represent a vast extension of the first (1927) edition, hardly any of which remains in its original form. Much credit is due to the team which was responsible for the translation of the 2nd edition for publication by the Blue Hill Meteorological Observatory. Despite the 3rd edition's advantage of years it is interesting to note that when the translation was nearing completion and the imminence of a new edition became known, Professor Geiger's aid was enlisted in providing additions to make the translation the virtual equivalent of the 3rd German edition.

The style is essentially descriptive and the frequent anecdotal excursions, many of them drawn from the author's own wide experience, make for interesting as well as informative reading. There is a broad division into two parts, the first dealing with the basic meteorological physics of the near-surface layers, the second with microclimatic effects in relation to topography, plants, animals and man. The minimum use of mathematics and the simplicity of the discussions of physical processes are such that the layman will be able to read much of the work with both enjoyment and profit.

The first part deals with the broad features of heat interchange in the surface layers, humidity and wind relationships, and the influence of the sub-surface medium. It engages briefly with acoustic and optical phenomena and with the transport of dust and radioactive emanations. The first section of the second part surveys topographical and exposure effects on air movement, sunshine, temperature and humidity. It then proceeds to the influence of plant and forest cover. In a final section chapters are devoted to various features of bioclimatology, the city climate and the frost problem. Qualitative and quantitative data are lavishly presented in tabular and diagrammatic form and, one of the most important features in a work of this kind, the bibliography is very detailed, running to nearly 1,000 references.

Although the translators may with all justification claim a virtual equivalence to the later German edition, there are differences in detail which may be of consequence to the specialist reader and research worker. The extra references to the published literature are of significance here and one feature of practical importance, the use of wind breaks, occupies a whole chapter in the 3rd Edition as compared with a few paragraphs in the translated 2nd Edition.

The third edition was commenced amid the difficulties of war. With this on top of language differences and the normal lag in the absorption of specific papers into collected text, it would not be surprising if some omissions of more recent developments are apparent. With regard to the fundamentals of the subject, for example, there have appeared in the literature of the last few years studies of diffusion of smoke, wind velocity profile and drag, evaporation and long-wave radiative exchange, which do not appear in the 3rd Edition. One point of specific interest in the discussion of wind-velocity distribution with height may lead to some confusion. The ' roughness height ' (*Rauhigkeitshohe*) as defined and numerically prescribed in Professor Geiger's book, is discussed in relation to the power-law variation of wind with height (not the logarithmic law), and appears to be equivalent to the parameter which in the English literature is usually given the name ' zero plane displacement.' But these are special points, whereas Professor Geiger's book covers a very wide range in a fashion and with a quality which leaves no doubt about its unique position in meteorological literature.

F.P.

Southeast Asia. By E. H. G. Dobby. London (University of London Press), 1950. 8vo. Pp. 415 ; 118 figures. 18s.

During the last war the term ' Southeast Asia ' came into everyday use to describe collectively the countries of Burma, Indochina and Malaya as well as the islands of the East Indies, including

orneo, and of the Philippines. Except in a fictional sense, these places were little known to most f us on this side of the world until the period following the war when the tasks of military occupation nd liberation gave large numbers of Britishers the opportunity of fairly extensive travel over South-ast Asia. Not even the most purblind could escape the fascination of both the region and its inhabi-nts. It was also not difficult to observe the tremendous work of development that had been ccomplished by European settlers.

It is a matter for surprise that comparatively little has been written about such a region and this ook by Professor Dobby is therefore specially to be welcomed. His object is to describe the general icture of Southeast Asia – its geography, climatic features and vegetation – and to show how the arious peoples, both indigenous and immigrant, have reacted or adapted themselves to their urroundings. In Part I the natural landscape of the region as a whole is portrayed and in Part II detailed discussion is given of each country or political unit. Finally in Part III entitled ' The uman Geography of Southeast Asia ' the problems facing the different peoples are explained and ends in industry and commerce are summarized. Political developments, or driftings, are also ppraised and if at times a note of warning appears to be sounded, that may be because the ostwar nationalist movement, involving as it does such diverse elements in regard to evolution and nguages, is something of a paradox.

It will be apparent that the scope of the book is very wide and in the space of some 400 pages he author has managed to achieve a quite comprehensive treatment. The work of selection and mission has obviously been done with particular care since only a very few topics seem to have ceived superficial attention. The book should appeal to all who are interested in the effects of nvironment on the development of mankind and especially to those who have visited Southeast sia or are likely to do so in the future.

P.J.M.

ontinentality and the analysis of atmospheric pressure over the northern hemisphere. By T. E. W. Schumann and M. P. van Rooy. Pretoria (Union of S. Africa Weather Bureau), 1950. 4vo. Pp. 21,8 ; tables, 10 charts. 5s.

Since the weather of any place depends to a very large extent on its situation in respect to the nd and sea distribution, climatologists have adopted the idea of ' continentality ' as one of the ndamentals of their science. Hitherto continentality has generally itself been measured in terms of me meteorological element such as annual range of temperature, but this is rather arguing in a rcle. It is more logical to define continentality solely in terms of geography. The authors of the emoir under consideration adopt this method, finding the total area of land, weighted for effect f distance, within a circle of 40° radius from every 10° intersection of latitude and longitude, with n allowance for ice. The results, on a scale from 0 to nearly 1,000, are tabulated and charted for the rthern hemisphere.

They next turn their attention to the mean monthly pressures at the same points, the collection f this body of homogeneous data being itself no mean contribution to meteorology. The data are nalysed in terms of annual means and amplitudes of sine and cosine terms of the annual semi-annual ariations. The values of these terms are correlated with the continentality and with its south-north adient. The result is a somewhat complicated function in which the pressure at any point in any nonth is represented by seven terms, a base dependent only on the latitude, the local continentality nd its gradient a variable distance to the west, a term combining continentality and annual variation, nd three purely seasonal terms. This expression is claimed to represent the actual pressure distribu-on fairly well, and would have done so better had an allowance been made for high ground, which stated to decrease effective continentality. The reviewer thinks that the anomaly over Greenland more probably due to the convention of regarding ice as land, instead of as a third form of surface, either land nor water, than to its height.

Besides giving a great amount of valuable data, a purpose of the investigation was to provide means of reconstructing the pressure distribution over the world in earlier geological periods. This xperiment is not actually tried, and the reviewer doubts if it would succeed in practice. The constants re determined for the earth as it exists to-day, and it seems unlikely that they would apply to a world f very different relief. Nevertheless, the experiment would be well worth trying by anyone with fficient leisure to undertake the lengthy calculations involved. It would certainly be no less real han some of the other mathematical reconstructions of the climate of past ages.

C.E.P.B.

Einführung in die Meteorologie auf physikalische Grundlage. By K. Schütt. Berlin (Palmen-Verlag), 1950. 8vo. Pp. 179 ; 86 figures. No price given.

When a scientist sits down to write a book about his subject he will usually have in mind some definite purpose to be served, some typical reader to be addressed, the professional colleague, the serious student in school or university or, that omnivorous reader, the man in the street. Dr. Schütt however does not reveal to us his objective but settles down straight away to a discourse about the atmosphere (the word comes from the Greek), its composition of oxygen (important for life) nitrogen (which forms nitric acid) and the inert gases (more inert than the inert metals gold, platinum, *etc.*) We quickly realise that we are indeed assumed to be a member of that lay public of this scientific age which derives a disinterested, aesthetic pleasure in wandering with a knowledgeable guide through any field of science.

Naturally such a work in German can expect no English-reading public and it would be out of place to enter upon a detailed review here. But we may perhaps remark that, in spite of the evident scientific competence of our author and the wealth of interesting information which he has put together for the delectation of his reader, touching lightly upon so much in meteorological instrumentation, physics, dynamics, synoptics and German climate, we should expect greater success with something less cut and dried. The author sounds just a little weary of his task and unable to inspire excitement with the modern progress of his science. And please will all writers of popular science texts give most careful attention to their diagrams, let them catch the reader's eye and provide them with captions which make them intelligible without reference to the text? Most of Dr. Schütt's diagrams are a model of how these things should not be done.

Nevertheless the reviewer must confess that in, so to speak, taking a busman's holiday with Dr. Schütt as guide, he was rarely bored and often pleasantly intrigued by the simple, yet for the most part accurate, presentation of basic physical ideas. The little experiments which are suggested to illustrate some of the principles are a useful and refreshing feature in a popular work. Apart from being offended by many of the illustrations, for example by thumb-nail sketches of warm sector depressions consisting of two concentric circles and two perpendicular radii, we were however a little impatient with the narrative style of conducting arithmetical calculations. Thus in an ordinary half-page paragraph, on p. 152, we talk our way by multiplications and divisions from the speed of the wind, through its mass and kinetic energy eventually to reach some hundred thousand men hewing coal for eight months to provide the heat equivalent. The continuous narrative of words and figures made the arithmetic quite difficult to check and it was not made the easier by this scientific curiosity 'A cubic metre (cbm) of air weighs about 1.3 kg, it has therefore the mass of 1.3 : 10 = 1/8 kg.' An Englishman's eye also caught the ' knot per hour ' as the unit of speed (p. 104). In this a landlubber's howler or an atrocity deliberately committed by the German Navy ?

This reviewer is not however ungrateful to the author, and if any younger meteorologist is looking for a beginner's German text containing much of his technical vocabulary but without the distraction of difficult scientific ideas, in fact for a primer in meteorological German, he might do much worse than make his way through Dr. Schütt's pages. He will, incidentally, be certain to learn many things, for example (p. 173), that nearly a thousand scientific investigations have been carried out with the object of finding a connection between the moon's phases and the weather – and with no notable success. The few misprints, such as giving the month 24 days in this paragraph, will only flavour the dish.

R.C.S.

Dating the past : an introduction to geochronology. Second edition revised. By F. E. Zeuner. London (Methuen), 1950. 8vo. Pp. xviii, 474 ; 24 plates, diagrams. 30s.

The first edition of this book was reviewed at length in the *Quarterly Journal* for January 1946. Since that date the bases of geochronology have been strengthened by the addition of further material and tested by criticism, while several promising new methods have been introduced. Part of this new material has been included in the text ; the remainder is given in a series of appended notes.

The book falls into four parts : tree rings ; varves or annual layers of sediment of glacial or lacustrine origin ; the succession of glacial and interglacial stages referred to changes of the earth's orbit ; and methods based on the products of radioactivity. Since one method takes up where another leaves off, and the datings agree sufficiently well at the overlaps, the chronological history of the earth can now be regarded as known, with some accuracy, for the past 1,500 million years, which appears to be about half of the period for which the earth itself has existed*.

* It is noted however that when we reach such vast ages there is some doubt as to whether the ordinary uniformly progressive time scale is applicable.

Recent work on tree rings and glacial varves tends to reject de Geer's assumption that the year-to-year variations of weather were similar in all parts of the world, and to build up independent time scales for the different regions or continents, a modification which all meteorologists will regard as a real advance. The application to Europe of historical dating by forming a connected series of tree rings in old beams, *etc.*, so successful in North America, should provide results of great interest if it proves to be practicable. It is possible too that more could be made of correlating distant series by taking less count of year-to-year variations and paying more attention to the lengths of the periodicities, especially those of solar origin. The new chemical methods of age determination, such as the fluorine and radio-carbon content, may also help in these prehistoric age-determinations.

Perhaps the most controversial part of the book is still that which relates the various stages of the Quaternary Ice Age to the dated astronomical sequence worked out by Milankovitch. The advances and retreats of the ice sheets in Europe and North America, dated approximately by geological methods, now fit well enough into the calculated wanings and waxings of solar radiation in the summer half-year to make it probable that the latter actually set off the glacial oscillations, though the Ice Age as a whole must be attributed to some other cause. But the attempts to fit the sequence of pluvial and dry periods in low latitudes into the scheme still appear forced and unsatisfactory. The comparatively slight effect of the astronomical changes near the equator is likely to have been completely masked by the repercussions of the great ice sheets of higher latitudes on the atmospheric circulation.

The book includes an exhaustive bibliography and is excellently produced. In these days of continually rising book costs the publishers are to be congratulated on having produced an enlarged edition at no increase of price. For the interest and information it presents, the volume is really good value for money.

C.E.P.B.

Crystal growth. By H. E. Buckley. New York (John Wiley), 1951. 8vo. Pp. xv, 571 ; 169 figures, 88 plates. $9.00.

In his book *Crystal growth* Dr. Buckley has provided the first comprehensive treatment in English of a subject whose growing importance meteorologists are coming to recognize. The book is written from 'a chemical point of view,' and the author cannot have had meterorologists in mind. There is not very much discussion of the principal crystal-growth problem in atmospheric physics, the growth of snowflakes and hailstones.

Nakaya's pre-war work on the classification and measurement of snowflakes observed at a mountain station in Japan is described in Chapter 8. But no mention is made of Weickman's very fine observations of the ice crystals observed in different types of ice cloud, though they were published just after the war. These observations were made during high-altitude aircraft flights, and are much more useful than Nakaya's, for they give some information about conditions in which the different types of ice crystal actually grow, and not just the conditions at the surface during a snowstorm. In Chapters 3-6 the author gives a good account of the two main theories of crystal growth. But neither the absorption-layer theory, nor the diffusion theory has led to satisfactory explanations of the rate of growth of real crystals, and this shortcoming is scarcely discussed.

It may be of interest to consider what a meteorologist would hope to find in a book on this subject. First and foremost, a review of the many papers on atmospheric ice crystals which are scattered through the literature, but now represent, in total, a quite considerable volume of knowledge. Secondly, a discussion of crystal growth from the vapour, particularly the growth of a freely-falling crystal, and of the factors which stimulate dendritic growth. Thirdly, a description of the methods which have been used to grow ice crystals artifically, both by sublimation, and by riming. And lastly a report of the present state of the theory of crystal growth, and of the attempts to reconcile the rates at which real crystals grow with the various theories. None of these branches of the subject is complete, and meteorologists will note how much work has still to be done before they become portions of a complete description of the formation and growth of atmospheric ice particles. But much more information is available than can be found in Dr. Buckley's book.

H.P.P.

Bibliography of regional meteorological literature. Vol. 1 *Southern Africa*, 1486–1948. R. J. Venter (Comp.). Pretoria (Government Printer), 1949. 4vo. Pp. 412. 21s.

The large number of publications now available in all branches of science is creating the corresponding difficulties of finding time to read current literature, obtaining copies of reprints which have direct applications even to a limited field in one branch of science and classifying them in such

a way that they are available for future reference. This is the first comprehensive bibliography dealing solely with southern Africa. It does not include non-scientific articles and weather reports appearing in magazines and newspapers. It is however a most voluminous publication.

In Pt. I the entries are given with a subject classification, combined with the region, and they are arranged according to the International Decimal classification for meteorological literature. For easy reference this classification is quoted and an index to it has been compiled. In Pt. II an author classification has been adopted and a brief summary of the contents of each paper is included. Statistical information is arranged chronologically for each region with a corresponding reference to the entry numbers in the bibliography.

The publication will form a most valuable book of reference for meteorological papers concerning southern Africa. Each entry is printed in both English and Afrikaans. The price must create envy in other countries.

R.S.R.

The principles of air navigation. By E. W. Anderson. London (Methuen), 1951. 8vo. Pp. xiii, 264 ; 261 diagrams. 25s.

In his foreword the author says that he has written this book on modern air navigation for the practical airman in an attempt to show how simple it all really is, to provide a not unnecessarily mathematical background of necessary theory and to show the principles underlying the various pieces of complicated equipment (' black boxes ') which are used nowadays for air navigation.

In general the author has been very successful and has produced a most readable book which abounds with concise and lucid explanations of things which one always regarded as difficult of demonstration. Examples are the section on errors in Chap. I and the section on map projections in Chap. II. Very occasionally the text is obscure but the diagrams and worked examples usually come to the rescue.

The handling of meteorology, which appears as background information on winds (Chap. VI), is not so satisfactory, sometimes perhaps, but not always, because the author has attempted to achieve brevity and simplicity in a subject which does not naturally lend itself to either. For example paragraph 26 describes how the airman can guess upper winds ' from the temperatures ' and ends : ' For this reason upper winds have been called thermals.' The section on pressure-pattern flying is sound as far as it goes but one would have expected to see a little more about the fairly elementary, but quite successful, techniques used at present on the N. Atlantic by civil operators.

In other parts of the book where meteorology enters indirectly there are sections which call for adverse comment. Examples are the obscure explanation on p. 76 of the effect of air temperature on altimeter readings and the suggestion on p. 77 that errors in pressure altimeters can be kept down to a few feet.

As might be expected there are some misprints in this first edition but most of them are obvious. The error in the size of the U.S. gallon on p. 125, is more surprising.

To sum up, this book can be recommended to meteorologists requiring a general survey of modern air navigation but it cannot be considered a safe source of meteorological background for practical airmen. It is clearly not intended for the specialist navigator.

G.J.W.O.

Recent additions to the Society's Library

E. W. Anderson, *Principles of air navigation*. London, Methuen, 1951. Pp. xiii, 264, 261 diagrams.

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D. W. Perrie, *Cloud physics*. Toronto, University of Toronto (Geoffrey Cumberlege), 1950. Pp. 119, 30 plates, 4 tables.

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Upper-atmosphere research reports. 6 parts. Washington, Naval Research Laboratory, 1946-1948. Pp. 8,11 ; i, 150 ; xi, 170 ; vi, 95 ; vi, 13. vi, 13.

Weather at the summit of Mount Fuji. No. 2. Hourly data. From July 1932 to December 1936. Tokyo, Central Meteorological Observatory, 1951. Pp. 324.

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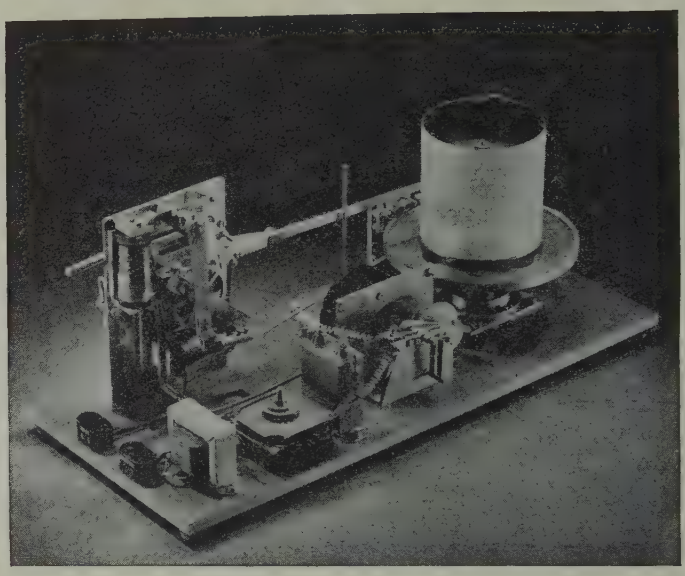

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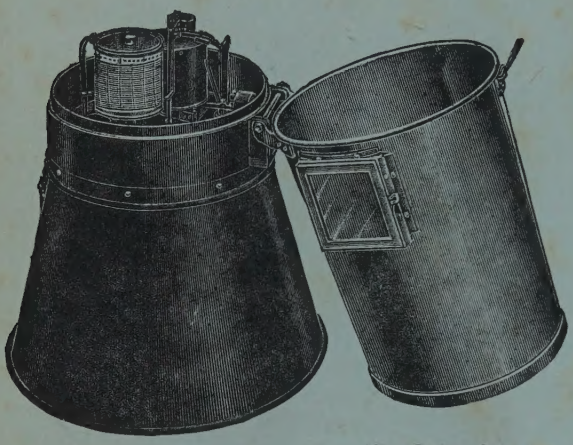

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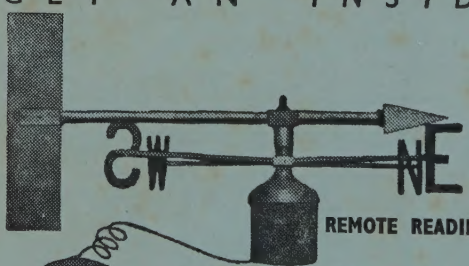

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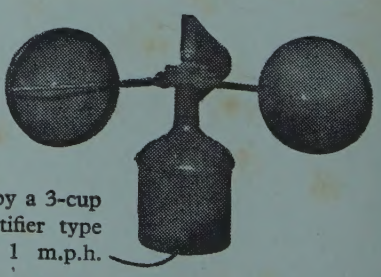

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